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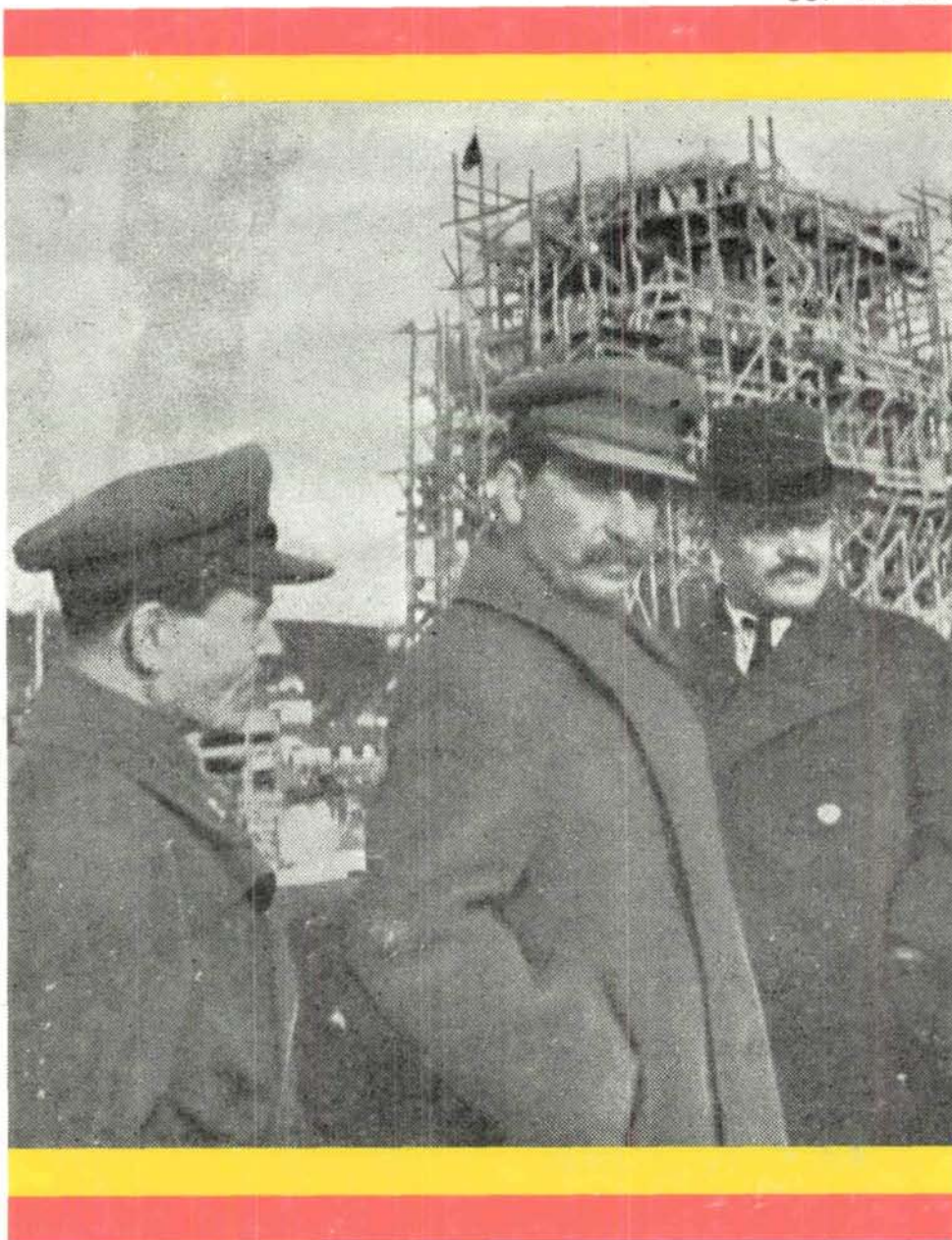
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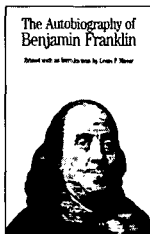
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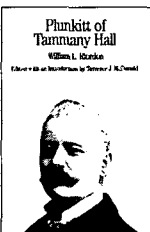
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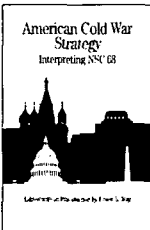
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Cover illustration: Joseph Stalin, shown here with his People's Commissar of Internal Affairs, N. I. Ezhov (left), who administered the infamous *Ezhovshchina* purge, and his Chairman of the Council of People's Commissars, V. M. Molotov (right), inspecting the building site at the construction of the Moscow-Volga canal (circa 1936). See the article in this issue, "Victims of the Soviet Penal System in the Pre-war Years: A First Approach on the Basis of Archival Evidence," by J. Arch Getty, Gábor T. Rittersporn, and Viktor N. Zemskov.



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In This Issue

For many years, scholars have been debating the magnitude of the Soviet system of repression under the rule of Joseph Stalin. Estimates of the number of people executed or swept into the penal system known as the GULAG have varied widely. Researchers with a strong animus against the USSR have figured the victims to be in the tens of millions. Less hostile investigators have offered far lower estimates. Without access to the police archives (or even adequate census counts), scholars have built their estimates on anecdotal evidence and untested assumptions about the operation of the criminal justice system and population dynamics. The lead article in this issue by **J. Arch Getty, Gábor T. Rittersporn, and Viktor N. Zemskov** is the result of the first explorations of the Stalinist penal system based on police and party documents in the Russian state archives. The authors acknowledge that the material they have found is not complete and is in some instances contradictory, yet its varied accounts do correspond to one another in rough orders of magnitude. These records support the lower estimates of victims proposed by investigators with a less hostile stance toward the Soviet Union.

In addition to presenting data on the number of victims, the essay looks at the structure of the pre-war Soviet penal system, the sentencing powers and habits of police agencies, and the social and age composition of prison inmates. The authors wrestle with the problem of defining “political” as distinguished from “common” criminals and engage in the difficult process of correlating records from a variety of judicial and penal institutions.

The decline of the United States’ share of the world rice market in the post-Civil War period has often been attributed to the effects of that war. **Peter A. Coclanis** suggests that a less parochial view of the history of this commodity tells a different story. In an analysis of the demand for rice in the West, he shows that the gradual expansion of capitalism and integration of markets internationally led to shifts in supply that had dramatic effects on various rice-producing regions, including those in the United States. He argues that these shifts are more persuasively explained by the actions of world markets than by the proximate causes that have usually been adduced for them.

James L. Huston makes a strong case for the staying power of the revolutionary generation’s ideas about political economy and points out that a radical break in American economic thought did not occur until the end of the nineteenth century. The Founders, although they did not write economic treatises, did have an underlying theory of the economy appropriate to a republic, namely one in which inequities of wealth were not strongly marked. To foster this type of economy, they had to decide what had created the inequities they noticed in European society; they concluded that these

inequities were created by an aristocratic society, which through taxation, political controls, entail and primogeniture, and establishment of the church, secured for the few the fruits of the labor of the many. Because the Republican generation's idea of a just distribution of wealth rested on a labor theory of value, they preferred laissez-faire policies that would let people enjoy the results of their own labor, assuming as they did that inequity arose out of political intervention. This anti-aristocratic view of economic policy not only persisted but gained in strength in the nineteenth century, affecting all major issues such as slavery and tariffs. Not until the rise of big business in the 1880s did Americans recognize that inequity arose from other causes, including most prominently economic monopoly.

The *AHR Forum* in this issue explores the question of European integration and its much-delayed development after a promising start at the beginning of this century. **Carl Strikwerda** contests the view that the era before World War I was dominated by a rising tide of nationalism and argues that international economic ties and interdependence were growing rapidly and equaled nationalism in importance. His discussion, which focuses on the iron and steel industry, challenges existing scholarship in four areas. First, Strikwerda notes that the growth of economic integration and multinational companies was not unilinear but ebbed and flowed over time. Second, he rejects the notion that finance capital alone drove economic integration before World War I; much of it was accomplished by truly multinational companies. Third, industrialists were not as close to nationalist elites, particularly in Germany, as many writers have suggested; big business accepted internationalism. Fourth, Strikwerda proposes that twentieth-century periodization be rethought in recognition of the strength of internationalism early in the century; the period from 1914 to the 1980s may be seen in the future as a deflection from a well-established trend toward integration.

Paul W. Schroeder praises Strikwerda's research but suggests that the roots of economic integration before World War I may not have gone as deep as Strikwerda believes. Schroeder also notes that integration can produce friction as well as cooperation, especially when the societies involved exhibit fragilities. More fundamentally, Schroeder questions the essay's conceptualization. He rejects the idea that there was a tension between internationalism and nationalism before the war and that nationalism won with the outbreak of the war. Schroeder argues instead that a consensus existed about how to succeed in international politics: a country required a strong military, based on a strong economy, which was in turn protected by the state's political and military forces in the international arena. War was pursued in part to preserve the international economy, not to destroy it. Although nationalism ultimately forced fragmentation, no one at the time perceived a conflict between nationalism and international cooperation. Increased integration would not

have lessened the likelihood of war so long as the competitive stances that sparked the war were still in play. States were not prepared to change the rules of the game. People were not convinced that there was another game to play, nor were they yet weary of the game of power politics.

Schroeder proposes a periodization different from that presented by Strikwerda. He sees the significant break in the mid-nineteenth century. Before the 1850s, power politics did not govern international relations; afterward, the trends Strikwerda examines came into play; events between 1914 and 1945 discredited power politics and permitted the revival of economic integration. Schroeder ends by noting the strength of Strikwerda's depiction of international relations as the result of human activity—not impersonal forces—and therefore a sphere in which learning occurs. We see the evidence in the recent instances of states resolving conflicts without resort to total war and exhaustion. This lesson is lost on some writers, who urge a return to power politics.

Carl Strikwerda offers a brief rebuttal.

The article section closes with a review article by **Marc Raeff**, discussing three recent biographies of successive rulers of Russia in the late eighteenth century by Carol S. Leonard, John T. Alexander, and Roderick E. McGrew. Raeff notes the neglect in the Soviet period of biographical studies of Russian rulers, despite the obvious importance of such persons in an autocratic regime. The fall of the Communist regime in Russia has brought a revival of the genre there, and in the West as well.

The October issue contains our annual **Film Review** section. This year, the contributing editor for film, **Robert A. Rosenstone**, has selected a menu of films from five continents. Reviewers evaluate the films for subject matter, technique, and especially for their success in treating issues of importance to historical studies.

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Contributors

Peter A. Coclanis is an associate professor of history and the associate dean of the College of Arts and Sciences at the University of North Carolina, Chapel Hill. He worked under Stuart W. Bruchey at Columbia University, earning his doctorate in 1984. He is the author of *The Shadow of a Dream: Economic Life and Death in the South Carolina Low Country, 1670–1920* (1989), as well as of numerous articles in economic and social history. Currently, he is writing a book on the history of rice. Coclanis spent the 1992–1993 academic year conducting research in Southeast Asia on a Fulbright Research Fellowship.

J. Arch Getty is a professor of history at the University of California, Riverside. He studied with Roberta Manning and received his Ph.D. from Boston College in 1979. He is the author of *Origins of the Great Purges: The Soviet Communist Party Reconsidered, 1933–1949* (1985) and co-editor of *Stalinist Terror: New Perspectives* (1993). His research is on the political history of the Soviet Union in the 1930s and concentrates on the history of the Soviet Communist Party. Getty is now writing (with Gábor Rittersporn) *Society and Politics in the Soviet 1930s* (forthcoming, Cambridge University Press), a treatment of the state-society question in the pre-war Stalin period, and is collaborating in the editing of a series of researchers' guides to Russian archives.

James L. Huston is an associate professor of history at Oklahoma State University. He received his doctorate in 1980 from the University of Illinois, Urbana-Champaign, under the guidance of Robert W. Johannsen. He is the author of *The Panic of 1857 and the Coming of the Civil War* (1987), which was subsequently awarded the Phi Alpha Theta prize for an author's first book. Although Huston has pursued a number of topics in political and economic history, his major concern has been an investigation of protectionist political economy. He is currently completing a book-length manuscript on this topic.

Marc Raeff is Bakhmeteff Professor Emeritus of Russian Studies at Columbia University.

He earned his doctorate in history from Harvard University in 1950. His recent books include *The Well-Ordered Police State: Social and Institutional Change through Law in the Germanies and Russia, 1600–1800* (1983), *Understanding Imperial Russia: State and Society in the Old Regime* (1984), and *Russia Abroad: A Cultural History of the Russian Emigration, 1919–1939* (1990).

Gábor T. Rittersporn is a senior research fellow at the Centre National de la Recherche Scientifique in Paris. He studied at the universities of Szeged (Hungary), Leningrad, and Tokyo, defending his doctoral dissertation at the Sorbonne in 1979. His research interests involve the interaction of collective representations, social practices, and political processes in the Soviet and post-Soviet periods, with particular emphasis on the evolution of penal policy. Rittersporn is the author of *Stalinist Simplifications and Soviet Complications: Social Tensions and Political Conflicts in the USSR, 1933–1953* (1991).

Paul W. Schroeder is professor of history and political science at the University of Illinois, Urbana-Champaign. He is the author of three books and many articles on the history of international politics from the eighteenth to the twentieth centuries, as well as Austrian and German history. His latest work, *The Transformation of European Politics, 1763–1848*, will be published by Oxford University Press in the *Oxford History of Modern Europe* series early in 1994. His current research is on change, development, and learning in international politics, 1648 to 1945.

Carl Strikwerda received his Ph.D. from the University of Michigan under the supervision of Louise A. Tilly and is now an associate professor of history at the University of Kansas. The co-editor with Camille Guerin-Gonzales, Oberlin College, of *The Politics of Immigrant Workers: Labor Activism and Migration in the World Economy since 1830* (1993), he is currently editing a volume with Ellen Furlough, Kenyon College, on the history of consumer cooperation. Strikwerda has had

articles published in *Comparative Studies in Society and History*, *International Labor and Working Class History*, and the *Journal of Urban History*, and recently completed a manuscript on Catholic and Socialist workers in Belgium between 1870 and 1914. His article in this issue grew out of research for a book on the conflict between nationalism and internationalism in the era of World War I.

Viktor N. Zemskov is a senior research fellow at the Institute of Russian History of the Russian Academy of Sciences. He received his *kandidat nauk* degree from the History Faculty of Moscow State University in 1974,

specializing in the history of the Soviet working class. He has written *The Leading Force of the National Struggle: The Struggle of the Soviet Working Class in the Period of Fascist Occupation of the USSR, 1941–1944* (in Russian) (1986). In 1989, Zemskov was among the first researchers admitted to the secret archives of the GULAG system, and he published a series of articles in *Argumenty i fakty* and *Sotsiologicheskie issledovaniia* on prisoners, exiles, and repatriation in the Stalin period. He is now preparing two books, one on Soviet citizens on forced labor in Nazi Germany, 1941–1945 and another on exiles in the USSR, 1930–1960.

Victims of the Soviet Penal System in the Pre-war Years: A First Approach on the Basis of Archival Evidence

J. ARCH GETTY, GÁBOR T. RITTERSPORN, and
VIKTOR N. ZEMSKOV

THE GREAT PURGES OF THE 1930s were a maelstrom of political violence that engulfed all levels of society and all walks of life. Often thought to have begun in 1934 with the assassination of Politburo member Sergei Kirov, the repression first struck former political dissidents in 1935–1936. It then widened and reached its apogee in 1937–1938 with the arrest and imprisonment or execution of a large proportion of the Communist Party Central Committee, the military high command, and the state bureaucracy. Eventually, millions of ordinary Soviet citizens were drawn into the expanding terror.¹

Debate in the West about the precise numbers of victims has appeared in the scholarly press for several years and has been characterized by wide disparity, often of several millions, between high and low estimates. Using census and other data, scholars have put forward conflicting computations of birth, mortality, and arrests in order to calculate levels of famine deaths due to agricultural collectivization (1932–1933), victims of the Great Terror (1936–1939), and total “unnatural” population loss in the Stalin period. Anton Antonov-Ovseenko, Robert Conquest, Steven Rosefelde, and others have posited relatively high estimates (see Table 1).² On the other hand, Stephen Wheatcroft and others working from

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¹ Standard works are Robert Conquest, *The Great Terror: Stalin's Purge of the Thirties* (New York, 1968); and *The Great Terror: A Reassessment* (New York, 1990); Roy A. Medvedev, *Let History Judge: The Origins and Consequences of Stalinism* (New York, 1989); Aleksandr I. Solzhenitsyn, *The Gulag Archipelago 1918–1956: An Experiment in Literary Investigation* (New York, 1973); J. Arch Getty, *Origins of the Great Purges: The Soviet Communist Party Reconsidered, 1933–1938* (New York, 1985); Robert W. Thurston, “Fear and Belief in the USSR's ‘Great Terror’: Response to Arrest, 1935–1939,” *Slavic Review*, 45, no. 2 (1986): 213–34; Gábor T. Rittersporn, *Stalinist Simplifications and Soviet Complications: Social Tensions and Political Conflicts in the USSR, 1933–1953* (Philadelphia, 1991); see also J. Arch Getty and Roberta T. Manning, eds., *Stalinist Terror: New Perspectives* (New York, 1993).

² For the most significant high estimates, see S. Rosefelde, “An Assessment of the Sources and Uses of Gulag Forced Labour, 1929–56,” *Soviet Studies*, 33, no. 1 (1981): 51–87; and “Excess Mortality

the same sources have put forth lower totals.³ Both “high” and “low” estimators have bemoaned the lack of solid archival evidence and have claimed that should such materials become available, they would confirm the author’s projection. The debate, along with disputes on the “totalitarian” nature of the Stalinist regime, the importance of Joseph Stalin’s personality, and the place of social history in Soviet studies, has polarized the field into two main camps, perhaps unfortunately labeled “Cold Warriors” and “revisionists.”⁴ Revisionists have accused the other side of using second-hand sources and presenting figures that are impossible to justify, while the proponents of high estimates have criticized revisionists for refusing to accept grisly facts and even for defending Stalin. Both sides have accused the other of sloppy or incompetent scholarship.

Now, for the first time, Soviet secret police documents are available that permit us to narrow sharply the range of estimates of victims of the Great Purges. These materials are from the archival records of the Secretariat of GULAG, the Main Camp Administration of the NKVD/MVD (the USSR Ministry of the Interior). They were housed in the formerly “special” (that is, closed) sections of the Central State Archive of the October Revolution of the USSR (TsGAOR), which is now part of the newly organized State Archive of the Russian Federation (GARF).⁵ A few Moscow scholars (among them V. N. Zemskov) had access to some of them in the past but were not allowed to cite them properly. Now, according to the liberalized access regulations in Russian archives, scholars are able to consult these documents and to publish exact citations.⁶ (See “A Note on Sources” at the end of this article.)

We propose to deal here only with quantitative elements of the terror, with what we can now document of the scale of the repression. Of course, such a cold numerical approach risks overshadowing the individual personal and psychological horror of the event. Millions of lives were unjustly taken or destroyed in the Stalin period; the scale of suffering is almost impossible to comprehend. The horrifying irrationality of the carnage involves no debatable moral questions—destruction of people can have no pros and cons. There has been a tendency to

in the Soviet Union: A Reconstruction of Demographic Consequences of Forced Industrialization, 1929–1949,” *Soviet Studies*, 35 (July 1983): 385–409; Robert Conquest, “Forced Labour Statistics: Some Comments,” *Soviet Studies*, 34 (July 1982): 434–39; and his *Great Terror: A Reassessment*, 484–89.

³ R. W. Davies and S. G. Wheatcroft, “Steven Rosefielde’s ‘Kliukva,’” *Slavic Review*, 39 (December 1980): 593–602; S. G. Wheatcroft, “On Assessing the Size of Forced Concentration Camp Labour in the Soviet Union, 1929–56,” *Soviet Studies*, 33, no. 2 (1981): 265–95; and “Towards a Thorough Analysis of Soviet Forced Labour Statistics,” *Soviet Studies*, 35, no. 2 (1983): 223–37; Jerry F. Hough and Merle Fainsod, *How the Soviet Union Is Governed* (Cambridge, Mass., 1979), 176–77; Barbara Anderson and Brian Silver, “Demographic Analysis and Population Catastrophes in the USSR,” *Slavic Review*, 44, no. 3 (1985): 517–36.

⁴ For a discussion of “revisionist” research, see Sheila Fitzpatrick, “New Perspectives on Stalinism,” *Russian Review*, 45, no. 4 (1986): 357–73; and the replies in *ibid.*, 375–413; and in *Russian Review*, 46, no. 4 (1987): 382–431.

⁵ Even though TsGAOR no longer exists, the GARF documents referenced here are numbered according to the old TsGAOR system. Because GARF now includes other formerly independent archives with their own numbering system, we cite numbered documents below as “GARF (TsGAOR).”

⁶ See *Vremennoe polozhenie: O poriadke dostupa k arkhivnym dokumentam i pravilakh ikh ispol'zovaniia* (Moscow, 1992), 3, 6, 8, for the new provisional rules of access.

accuse "low estimators" of somehow justifying or defending Stalin (as if the deaths of 3 million famine victims were somehow less blameworthy than 7 million).

Scholars and commentators will make use of the data as they choose, and it is not likely that this new information will end the debates. Still, it seems a useful step to present the first available archival evidence on the scale of the Great Terror. Admittedly, our figures are far from being complete and sometimes pose almost as many questions as they answer. They nevertheless give a fairly accurate picture of the orders of magnitude involved and show the possibilities and limits of the data presently available.

THE PENAL SYSTEM ADMINISTERED BY THE NKVD (Peoples' Commissariat of Internal Affairs) in the 1930s had several components: prisons, labor camps, and labor colonies, as well as "special settlements" and various types of non-custodial supervision. Generally speaking, the first stop for an arrested person was a prison, where an investigation and interrogation led to conviction or, more rarely, release.⁷ After sentencing, most victims were sent to one of the labor camps or colonies to serve their terms. In December 1940, the jails of the USSR had a theoretical prescribed capacity of 234,000, although they then held twice that number.⁸ Considering this—and comparing the levels of prison populations given in the Appendixes for the 1930s and 1940s—one can assume that the size of the prison system was probably not much different in the 1930s.⁹

Second, we find a system of labor camps. These were the terrible "hard regime" camps populated by dangerous common criminals, those important "politicals" the regime consigned to severe punishment, and, as a rule, by other people sentenced to more than three years of detention.¹⁰ On March 1, 1940, at the end of the Great Purges, there were 53 corrective labor camps (*ispravitel'no-trudovye lageri*: ITL) of the GULAG system holding some 1.3 million inmates. Most of the data cited in this article bear on the GULAG camps, some of which had a multitude of subdivisions spreading over vast territories and holding large numbers of people. BAMLAG, the largest camp in the period under review, held more than 260,000 inmates at the beginning of 1939, and SEVVOSTLAG (the notorious Kolyma complex) some 138,000.¹¹

Third came a network of 425 "corrective labor colonies" of varying types. These

⁷ Release became increasingly rare in the 1930s. Even though the number of convicts in the Russian Federation declined from more than 2 million in 1933 to 1,217,309 in 1935, the proportion of custodial sentences increased from 24.3 percent in 1933 to 37.8 percent in 1935 to 44 percent by the first six months of 1936; GARF (TsGAOR), fond 9474, opis' 1, delo 97, listy 19, 59; and delo 104, list 8. (Subsequent archival citations will use abbreviations: f. = fond, op. = opis', d. = delo, l. and ll. = list and listy.)

⁸ GARF (TsGAOR), f.9474, op.1, d.6, l.123.

⁹ It must be noted, however, that in May 1933, 800,000 inmates were held "at places of detention . . . with the exception of camps." The all-time high came in early 1938, when 910,307 people were held in such places (548,756 of them in prisons, notwithstanding a theoretical "limit" of 155,439 places). "Smolensk Archive," WKP 178, 134; GARF (TsGAOR), f.9414, op.1, d.1139, l.88.

¹⁰ This rule must have changed over the years, because the proportion of labor camp detainees serving terms of less than three years exceeded 18 percent by 1940 and 28 percent in January 1941; GARF (TsGAOR), f.9414, op.1, d.1155, l.7.

¹¹ GARF (TsGAOR), f.9414, op.1, d.1155, l.20.

colonies were meant to confine prisoners serving short sentences, but this rule varied with time.¹² The majority of these colonies were organized to produce for the economy and housed some 315,000 persons in 1940. They were nevertheless under the control of the NKVD and were managed—like the rest of the colony network—by its regional administrations. Additionally, there were 90 children's homes under the auspices of the NKVD.¹³

Fourth, there was the network of "special resettlements." In the 1930s, these areas were populated largely by peasant families deported from the central districts as "kulaks" (well-to-do peasants) during the forced collectivization of the early 1930s. Few victims of the Great Purges of 1936–1939 were so exiled or put under other forms of non-custodial supervision: in 1937–1938, only 2.1 percent of all those sentenced on charges investigated by the political police fell into this category.¹⁴ This is why we will not treat exile extensively below.

Finally, there was a system of non-custodial "corrective work" (*ispravitel'no-trudovye raboty*), which included various penalties and fines. These were quite common throughout the 1930s—they constituted 48 percent of all court sentences in 1935¹⁵—and the numbers of such convictions grew under the several laws on labor discipline passed on the eve of the war. Typically, such offenders were condemned to up to one year at "corrective labor," the penalty consisting of work at the usual place of one's employment, with up to 25 percent reduction of wage and loss of credit for this work toward the length of service that gave the right to social benefits (specific allocations, vacation, pension).¹⁶ More than 1.7 million persons received such a sentence in the course of 1940 and almost all of them worked in their usual jobs "without deprivation of freedom."¹⁷ As with resettlements, this correctional system largely falls outside the scope of the Great Terror.

Figure A provides the annual totals for the detained population (GULAG camps, labor colonies, and "kulak" resettlements, minus prisons) in the years of the Great Purges. It shows that, despite previously accepted—and fairly inflated—figures to the contrary, the total camp and exile population does not seem to have exceeded 3.5 million before the war. Were we to extrapolate from the fragmentary prison data we do have (see the Appendixes), we might reasonably add a figure of 300,000–500,000 for each year, to put the maximum total detained population at around 3 million in the period of the Great Purges.¹⁸

¹² Some 17.9 percent of the political prisoners and 41.7 percent of those convicted for the theft of public property were held in colonies, not camps, by 1951, although the overwhelming majority of them were serving terms of more than five years; GARF (TsGAOR), f.9414, op.1, d.1356, ll.1–3.

¹³ The 1940 data on the camps, colonies, and children's homes come from GARF (TsGAOR), f.9414, op.1, d.28, ll.2–3.

¹⁴ Between 1930 and 1936, the figure had been 32.6 percent; GARF (TsGAOR), f.9401, op.1, d.4157, ll.202–03. Detailed statistical information on the resettlements can be found in V. N. Zemskov, "Spetsposelentsy: Po dokumentatsii NKVD-MVD SSSR," *Sotsiologicheskie issledovaniia*, no. 11 (1990): 3–17. The numbers of "special settlers" quoted below come from this article.

¹⁵ GARF (TsGAOR), f.9474, op.1, d.97, l.19.

¹⁶ *Ugolovnyi kodeks RSFSR—Kommentarii* (Moscow, 1944), 36–38.

¹⁷ GARF (TsGAOR), f.8131sch, op.27, d.540, ll.9–22; f.9492, op.6, d.14, ll.10–11. In this and certain other categories of punishment, it was possible to be sentenced without having been arrested.

¹⁸ See GARF (TsGAOR), f.9414, op.1, d.1139, l.88, for what is likely to be the record number of prison inmates at the beginning of 1938, and GARF (TsGAOR), f.9401, op.1, d.4157, ll.202, 203–05,

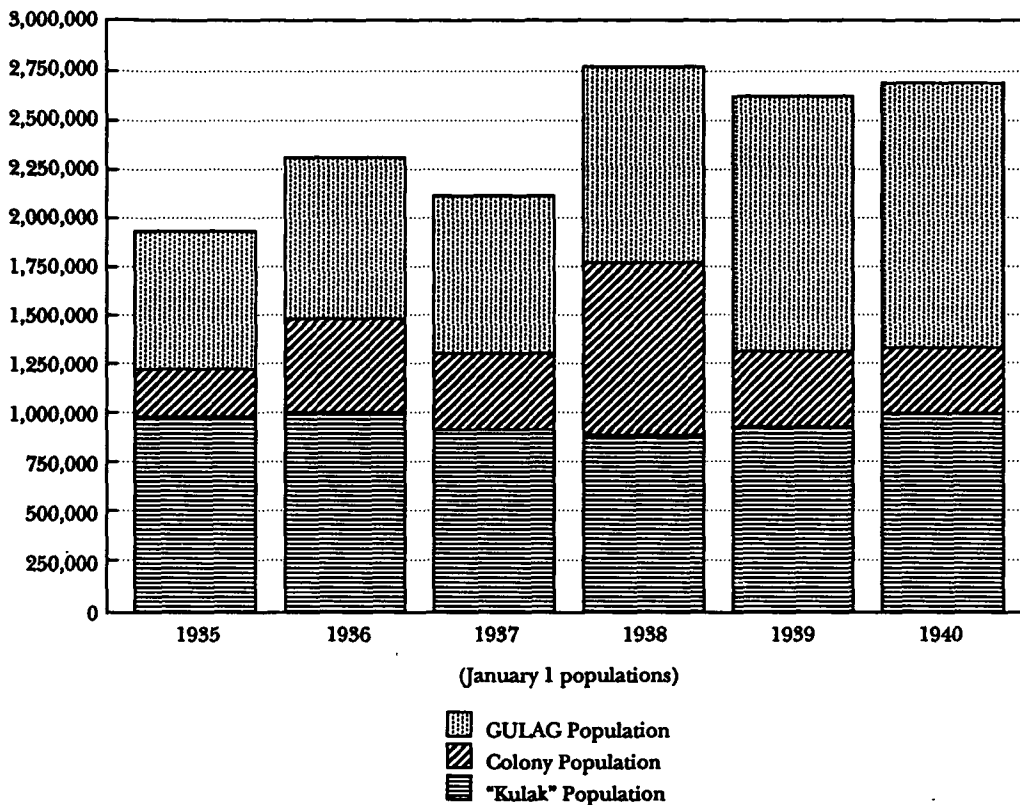


FIGURE A: Camp, Colony, and "Kulak" Exile Populations, USSR, 1935–1940

Mainstream published estimates of the total numbers of "victims of repression" in the late 1930s have ranged from Dmitrii Volkogonov's 3.5 million to Ol'ga Shatunovskaia's nearly 20 million. (See Table 1.) The bases for these assessments are unclear in most cases and seem to have come from guesses, rumors, or extrapolations from isolated local observations. As the table shows, the documentable numbers of victims are much smaller.

We now have archival data from the police and judiciary on several categories of repression in several periods: arrests, prison and camp growth, and executions in 1937–1938, and deaths in custody in the 1930s and the Stalin period generally. Runs of data on arrests, charges, sentences, and custodial populations in the 1930s unfortunately reflect the simultaneous actions of several punitive agencies including the secret police, procuracy, courts, and others, each of which kept their own records according to their own statistical needs. No single agency (not even the secret police) kept a "master list" reflecting the totality of repression. Great

for figures on exile, which may nevertheless contain a certain number of people banished in the wake of collectivization. Even though the number of exiles other than "kulaks" was relatively significant until the mid-1930s, it decreased to around 28,000 by the end of the decade. "Spravka ob administrativno-ssyl'nykh i vyslannykh, sostoiashchikh na uchte v organakh NKVD–UNKVD s 1-go avgusta 1939 g. po 1 ianvaria 1940 g.," document photocopied in the Archive of the USSR Ministry of the Interior by the society Memorial, to which we are indebted for having put it at our disposal.

Table 1. Current Estimates of the Scale of Stalinist Repression

	1937-38 total arrests	1938 camp population	1938 prison and camp population	1952 camp population	1937-38 camp deaths	1937-38 executions	1921-53 executions
Anton Antonov-Ovseenko	18.8 million ¹		16 million				7 million
Roy A. Medvedev	5-7 million					0.5-0.6 million	
Ol'ga Shatunovskaia	19.8 million ¹					7 million	
Dmitri Volkógonov	3.5-4.5 million						
Robert Conquest	7-8 million	~7 million	~8 million	12 million	2 million	1 million	
Documentable	~2.5 million	1.9 million²	2.0 million²	2.5 million²	160,084³	681,692	799,455⁴

NOTES:

¹1935-1940²includes labor colonies³in GULAG (hard regime) camps, in labor colonies, and in prisons (For the latter, see GARF [TsGAOR], fond 9414, opis' 1, delo 2740, list 52.)⁴in cases initiated or investigated by police agencies that include perhaps the majority of people sentenced for "political offenses"Another source indicates 786,096 executions for "counterrevolutionary crimes" between 1930 and 1953 (*Pravda* [February 14, 1990]: 2).SOURCES: A. Antonov-Ovseenko, *The Time of Stalin: Portrait of a Tyranny* (New York, 1980), 212; Roy A. Medvedev, *Let History Judge: The Origins and Consequences of Stalinism*, rev. edn. (New York, 1989), 455; *Moskovskie novosti*, November 27, 1988; O. Shatunovskaia, "Fal'sifikatsiia," *Argumenty i fakty*, no. 22 (1990); Robert Conquest, *The Great Terror: A Reassessment* (New York, 1990), 485-86; GARF (TsGAOR), fond 9401, opis' 1, delo 4157, listy 201-02; see also Appendixes and Note on Sources, below.

care is therefore needed to untangle the disparate events and actors in the penal process.

A 1953 statistical report on cases initiated or investigated by the NKVD provides data on arrests and on the purported reasons for them. According to these figures, 1,575,259 people were arrested by the security police in the course of 1937-1938, 87.1 percent of them on political grounds. Some 1,344,923, or 85.4 percent, of the people the secret police arrested in 1937-1938 were convicted.¹⁹ To be sure, the 1,575,259 people in the 1953 report do not comprise the total of 1937-1938 arrests. Court statistics put the number of prosecutions for infractions unrelated to "counterrevolutionary" charges at 1,566,185,²⁰ but it is unlikely that all persons in this cohort count in the arrest figures. Especially if their sentence was non-custodial, such persons were often not formally arrested. After all, 53.1 percent of all court decisions involved non-custodial sentences in 1937 and 58.7

¹⁹ GARF (TsGAOR), f.9401, op.1, d.4157, ll.203, 205. The contrast is striking with the period 1930-1936, when 61.2 percent were arrested for political reasons, and 61.7 percent of all those arrested by the political police were eventually convicted, and especially with the years from 1920 through 1929, when 58.7 percent of security police arrests were for political reasons, but only 20.8 percent of all those arrested were convicted. A handwritten note on this document tells us that 30 percent of those sentenced between 1921 and 1938 "on cases of the security police" were "common criminals," and their number is given as 1,062,000. Since the report speaks of 2,944,879 convicts, this figure constitutes 36 percent; 30 percent would amount to 883,464 persons (l.202).

²⁰ GARF (TsGAOR), f.9492, op.6, d.14, l.14.

percent in 1938, and the sum total of those who were executed or incarcerated yields 647,438 persons in categories other than "counterrevolution." ²¹ Even if we remember that during the Great Purges the authorities were by far more inclined to detain suspects than in other times, it seems difficult to arrive at an estimate as high as 2.5 million arrests on all charges in 1937–1938.

Although we do not have exact figures for arrests in 1937–1938, we do know that the population of the camps increased by 175,487 in 1937 and 320,828 in 1938 (it had declined in 1936). The population of all labor camps, labor colonies, and prisons on January 1, 1939, near the end of the Great Purges, was 2,022,976 persons. ²² This gives us a total increase in the custodial population in 1937–1938 of 1,006,030. Nevertheless, we must add to these data the number of those who had been arrested but not sent to camps, either because they were part of a small contingent released sometime later or because they were executed.

As Table 1 shows, popular estimates of executions in the Great Purges of 1937–1938 vary from 500,000 to 7 million. We do not have exact figures for the numbers of executions in these years, but we can now narrow the range considerably. We know that between October 1, 1936, and September 30, 1938, the Military Board of the Supreme Court, sitting in 60 cities and towns, sentenced 30,514 persons to be shot. ²³ According to a press release of the KGB, 786,098 persons were sentenced to death "for counterrevolutionary and state crimes" by various courts and extra-judicial bodies between 1930 and 1953. ²⁴ It seems that 681,692 people, or 86.7 percent of the number for this 23-year-period were shot in 1937–1938 (compared to 1,118 persons in 1936). ²⁵ A certain number of these unfortunates had been arrested before 1937, including exiled and imprisoned ex-oppositionists who were summarily killed in the autumn of 1937. ²⁶ More important, however, our figures on 1937–1938 executions are not entirely comparable to those quoted in the press release. Coming from a 1953 statistical report "on the quantity of people convicted on cases of NKVD bodies," they also refer to victims who had not been arrested for political reasons, ²⁷ whereas the communiqué concerns only persons persecuted for "counterrevolutionary offenses." In any event, the data available at this point make it clear that the number shot in the two worst purge years was more likely a question of hundreds of thousands than of millions. ²⁸

²¹ Calculated on the basis of GARF (TsGAOR), f.9492, op.6, d.14, l.29, by subtracting the number of "counterrevolutionaries" indicated on l.14. The actual figure is nevertheless somewhat smaller, since the data on death sentences include "political" cases.

²² Unless otherwise noted, data quoted in the text are drawn from the Appendixes, "USSR Custodial Populations, 1934–1953."

²³ Dmitrii Volkogonov, *Triumf i tragediia: Politicheskii portret I. V. Stalina* (Moscow, 1989), vol. 1, part 2, 246.

²⁴ *Pravda* (February 14, 1990): 2.

²⁵ *Pravda* (June 22, 1989): 3; *Kommunist*, no. 8 (1990): 103; GARF (TsGAOR), f.9401, op.1, d.4157, l.202.

²⁶ *Izvestiia TsK KPSS*, no. 10 (1989): 75, 77–78; no. 1 (1990): 52–53.

²⁷ "Spravka o kolichestve osuzhdennykh po delam organov NKVD"; GARF (TsGAOR), f.9401, op.1, d.4157, l.202. Judiciary statistics mention 4,387 death sentences pronounced by ordinary courts in 1937–1938, but this figure also includes a certain number of "political" cases; GARF (TsGAOR), f.9492, op.6, d.14, l.29.

²⁸ The only period between 1930 and the outbreak of the war when the number of death sentences for non-political crimes outstripped the ones meted out to "counterrevolutionaries" was

Of course, aside from executions in the terror of 1937–1938, many others died in the regime's custody in the decade of the 1930s. If we add the figure we have for executions up to 1940 to the number of persons who died in GULAG camps and the few figures we have found so far on mortality in prisons and labor colonies,²⁹ then add to this the number of peasants known to have died in exile, we reach the figure of 1,473,424. To be sure, of 1,802,392 alleged kulaks and their relatives who had been banished in 1930–1931, only 1,317,022 were still living at their places of exile by January 1, 1932. (Many people escaped: their number is given as 207,010 only for the year of 1932.)³⁰ But even if we put at hundreds of thousands the casualties of the most chaotic period of collectivization (deaths in exile, rather than from starvation in the 1932 famine), plus later victims of different categories for which we have no data, it is unlikely that "custodial mortality" figures of the 1930s would reach 2 million: a huge number of "excess deaths" but far below most prevailing estimates. Although the figures we can document for deaths related to Soviet penal policy are rough and inexact, the available sources provide a reliable order of magnitude, at least for the pre-war period.

Turning to executions and custodial deaths in the entire Stalin period, we know that, between 1934 and 1953, 1,053,829 persons died in the camps of the GULAG. We have data to the effect that some 86,582 people perished in prisons between 1939 and 1951.³¹ (We do not yet know exactly how many died in labor colonies.) We also know that, between 1930 and 1952–1953, 786,098 "counter-revolutionaries" were executed (or, according to another source, more than 775,866 persons "on cases of the police" and for "political crimes").³² Finally, we know that, from 1932 through 1940, 389,521 peasants died in places of "kulak" resettlement.³³ Adding these figures together would produce a total of a little more than 2.3 million, but this can in no way be taken as an exact number. First of all, there is a possible overlap between the numbers given for GULAG camp deaths and "political" executions as well as between the latter and other victims of the 1937–1938 mass purges and perhaps also other categories falling under police jurisdiction. Double-counting would deflate the 2.3 million figure. On the other hand, the 2.3 million does not include several suspected categories of death in custody. It does not include, for example, deaths among deportees during and

from August 1932 to the last quarter of 1933. This year saw the heavy-handed application of a particularly harsh decree against the theft of public property (the "Law of August 7, 1932"), and 5,338 people were condemned to death under its terms in 1932 and a further 11,463 in 1933; GARF (TsGAOR), f.9474, op.1, d.76, l.118; d.83, l.5. It is highly probable that far from all these people were executed (d.97, ll.8, 61). At any rate, the campaign began to lose its momentum by the closing months of 1933. On the uncertainty of our 1932–1933 data on thieves of public property, see below.

²⁹ At least 69,566 deaths were recorded in prisons and colonies between January 1935 and the beginning of 1940; GARF (TsGAOR), f.9414, op.1, d.2740, ll.52, 60, 74. The other data are 288,307 for strict regime camps and 726,030 for people executed "on cases of the political police."

³⁰ Zemskov, "Spetsposelentsy," 6; A. N. Dugin, "Neizvestnyi Gulag: Dokumenty i fakty," unpublished manuscript, 112.

³¹ The available records do not include a figure for 1945. And 76.6 percent of these victims fall to the war years. V. N. Zemskov, "Gulag: Istoriko-sotsiologicheskii aspekt," *Sotsiologicheskie issledovaniia*, no. 7 (1991): 7.

³² *Pravda* (February 14, 1990): 2; GARF (TsGAOR), f.9401, op.1, d.4157, ll.201–03, 205.

³³ See Zemskov, "Spetsposelentsy," 6, for detailed data on exiled "kulak" populations.

Table 2. Age and Gender Structure of GULAG Population
(as of January 1 of each year)

	Percent of GULAG Population			Percent of USSR Population	
	1934	1937	1940	January 1937	January 1939
AGE/SEX					
up to 18 years of age	1.2	0.7	0.5*	5.0‡	—
19–24	23.8	12.0	9.6	10.3	—
25–30	26.2	47.0	34.8	11.7	33.0
31–40	28.1	26.3	30.0	13.8	—
41–50	16.0	10.7	16.7	8.7	9.0
50+	4.7	3.3	8.4†	11.9	13.0
Women	5.9	6.1	8.1	52.7	—

NOTES: 1939 categories do not exactly match those of 1937. Respectively, the 1939 groupings are: 20–39, 40–49, and 50+.

*close to 1.2 percent by March 1940 (see GARF [TsGAOR], fond 9414, opis' 1, delo 28, list 14) and 4.5 percent by January 1, 1941

†7 percent for the age group of 51–60 versus 3 percent in 1937 and 4.5 percent in 1934—6.2 percent of the USSR population as of January 1, 1937

‡ages 16–18 (ages 12–15 = 7.5 percent)

SOURCES: GARF (TsGAOR), fond 9414, opis' 1, delo 1155, listy 9–10 (camp population); Rossiiskii gosudarstvennyi arkhiv ekonomiki (RGAE) [TsGANKH], f. 1562, op. 329, d. 144, ll. 2–10 (1937 data); Frank Lorimer, *The Population of the Soviet Union* (Geneva, 1946), 143 (1939 data—estimated distributions).

after the war as well as among categories of exiles other than “kulaks.”³⁴ Still, we have some reason to believe that the new numbers for GULAG and prison deaths, executions as well as deaths in peasant exile, are likely to bring us within a much narrower range of error than the estimates proposed by the majority of authors who have written on the subject.

WE NOW HAVE SOME INFORMATION about the demographic composition of the GULAG's prisoners. In terms of gender, there are few surprises. As Table 2 shows, women constituted a minority of hard regime camp inmates, although their share reached almost 13 percent by 1943 and 24 percent by 1945. They accounted for no more than 11 percent of the people prosecuted by the court system until the late 1930s, then the demographic situation of the war years increased their part to more than 40 percent by 1944; and, even though this proportion diminished afterward, it did not descend below 20 percent until 1955.³⁵

As we look at Table 2, the prominence of persons between 25 and 40 years of age among labor camp inmates is not surprising. A shift can be observed between 1934 and 1940. The generation that grew up in the tumult of war, civil war, and

³⁴ To mention only one example, we have information to the effect that 17 percent of Crimean Tatars who had been banished to Uzbekistan died before the end of 1945, some 27,000 people. A. Nekrich, *The Punished Peoples: The Deportation and Fate of Soviet Minorities at the End of the Second World War* (New York, 1978), 113–15.

³⁵ GARF (TsGAOR), f.9492, op.6, d.14, l.19; f.9474, op.1, d.97, l.6.

revolution and came of age in the New Economic Policy era continued to constitute a cohort more exposed to penal sanctions than the rest of society. Thus people between ages 19 and 24 in 1934 are likely to account for the large over-representation of the age group 25 to 30 in 1937 and of the 31 to 35 cohort on the eve of the war. Those in the 51 to 60 and especially 41 to 50 age ranges, however, seem to be most vulnerable to repression in the wake of crises like collectivization and the Great Purges. The presence of persons between ages 18 and 21 also becomes notable in the camps by March 1940, when they made up 9.3 percent of the inmates (their share in the 1937 population was 6.4 percent).

In fact, it gives one pause to reflect that 1.2 percent of strict regime camp detainees were 18 or younger in 1934 and that, by 1941, their share nearly reached the proportion of those between 16 and 18 in the country's population. From mid-1935 to the beginning of 1940, 155,506 juveniles between the ages of 12 and 18 passed through the labor colonies. Some 68,927 of them had been convicted of a crime and 86,579 had not.³⁶ The large proportion of unconvicted young detainees indicates that they were likely to be incarcerated by extra-judicial bodies, as was a high proportion of adult inmates not sentenced by courts between 1938 and 1940.³⁷ Nevertheless, political reasons did not play a predominant role in the conviction of minors. The ordeal of collectivization and the ensuing famine as well as the turmoil of mass migration from countryside to cities dramatically increased the number of orphans, abandoned children, and single-parent households and weakened the family as well as the social integration of some categories of youth. Juvenile delinquency became a serious concern for the authorities by the spring of 1935, when they ordered that the courts were entitled to apply "all penal sanctions" to children having reached 12 years and guilty of "theft, violence, bodily harm, mutilation, murder and attempted murder."³⁸

Records show that 10,413 youngsters between 12 and 16 years of age were sentenced by the courts of the Russian Federation in the second half of 1935 and the first half of 1936; 77.7 percent of them were accused of theft (as opposed to 43.8 percent of those in the 16 to 18 group) and 7.1 percent of violent crimes.³⁹ At this time, when the overall proportion of custodial sentences did not exceed 44 percent in the republic, 63.5 percent of the youngest offenders (and 59.4 percent between 16 and 18) were sent to detention.⁴⁰ In addition, there was a tendency to apply the 1935 decree to infractions it did not cover; thus, despite instructions to the contrary, 43 juveniles were sentenced for alleged misconduct in office [!] by mid-1936 and 36 youngsters under 16 were so sentenced between 1937 and

³⁶ GARF (TsGAOR), f.9414, op.1, d.28, l.15. The latter category of juveniles in custody but not convicted of a crime may represent in part the children of arrested "enemies of the people." Some 13,172 family members of alleged "traitors to the Motherland" were held in GULAG camps alone as of January 1, 1939; GARF (TsGAOR), f.9414, op.1, d.1155, l.4.

³⁷ See Table 8.

³⁸ *Sobranie zakonov i rasporyazhenii Raboche-Krest'ianskogo Pravitel'stva SSSR*, chast' I (1935), 262; A. Shliapochkinov, "Prestupnost' i repressii v SSSR," *Problemy ugolovnoi politiki*, kn. 1 (Moscow, 1935), 80; *Ugolovnyi kodeks RSFSR* (Moscow, 1937), 105; *KPSS v rezolutsiakh i resheniakh s'ezdov, konferentsii i plenumov TsK*, vol. 5 (Moscow, 1971), 206–11.

³⁹ Compared to an analogous 7.7 percent of convictions among their elders for violent crimes; GARF (TsGAOR), f.9474, op.16, d.79, ll.45, 73.

⁴⁰ GARF (TsGAOR), f.9474, op.1, d.97, l.6; d.104, l.8.

Table 3. Data on 10,366 Juvenile Camp Inmates, April 1, 1939*

	No.	Percent of All Sentences	Adults: Percent of All Sentences January 1, 1939
<i>Sentenced for:</i>			
"Counterrevolutionary offenses"	160	1.6	34.5
Dangerous crimes against the administrative order, including Banditry	929	9.0	14.8
Misconduct in office	60	0.6	6.1
Crimes against persons	434	4.2	4.8
Crimes against property	2,507	24.4	12.1
Theft of public property†	22	0.2	2.1
Being "socially harmful and dangerous elements"‡	5,838	56.9	21.7
Violating the law on internal passports	115	1.1	2.1
Other crimes	204		

NOTES:

*of a total of 10,371 juveniles in the GULAG system.

†In reality, a great number of thieves of public property were not sentenced under the terms of the decree sanctioning this type of crime.

‡The meaning of this category is explained in the text below.

SOURCES: GARF (TsGAOR), fond 9414, delo 1140, opis' 1, listy 151, 153; d. 1155, ll. 3–6, 9 (see also d. 1140, ll. 190, 193–94, for somewhat different proportions concerning adults on January 1, 1939).

1939.⁴¹ The sources show, incidentally, that the procuracy suggested that people below 18 years of age should not be confined in ordinary places of detention, and there is reason to believe that it also vainly protested against a directive of the camp administration stipulating that "the stay of minors in labor colonies is not limited by the terms of court sentences."⁴²

At any rate, 24,700 children and adolescents up to 16 years of age appeared in courts in 1938 and 33,000 in the course of the following year,⁴³ an increase that reflects a hardening penal practice. Table 3 indicates, however, that even if juveniles could be detained for political reasons, this motive did not account for a high proportion of the youngest camp inmates, even in the wake of the Great Purges. Although these data denote a tendency to imprison juveniles almost in the same proportions as adults if they were accused of the most serious crimes, they also show the penal system's proclivity to impose custodial sentences on youngsters more readily than on grown-ups.

Table 4 shows the national origin of the majority of labor camp inmates on January 1, 1937–1940, alongside the ethnic composition of the USSR according to the working materials of the (suppressed) 1937 and (published) 1939 censuses. In comparison with their weight in the general population, Russians, Belorussians, Turkmen, Germans, and Poles were over-represented in the camps by 1939;

⁴¹ *Ugolovnyi kodeks RSFSR* (1937), 105; GARF (TsGAOR), f.9492, op.6, d.14, l.23; f.9474, op.16, d.79, l.45.

⁴² GARF (TsGAOR), f.8131sch, op.27, d.71, ll.104–05.

⁴³ GARF (TsGAOR), f.8131sch, op.27, d.239, ll.115–16; 78.1 percent of them were convicted of theft and 5.3 percent of robbery.

Table 4. Ethnic Groups in GULAG Camps, January 1, 1937–1940

Ethnic Group	1937	1938	1939	1940	1937 camps %	1937 census %	1939 camps %	1939 census %	over (+)/under (–) representation (camps and census)	
									1937	1939
Russians	494,827	621,733	830,491	820,089	60.28	58.07	63.05	58.09	+2.21	+4.96
Ukrainians	138,318	141,447	181,905	196,283	16.85	16.33	13.81	16.47	+0.52	–2.66
Belorussians	39,238	49,818	44,785	49,743	4.78	3.01	3.40	3.09	+1.57	+0.31
Tatars	—	22,916	24,894	28,232	—	1.35	1.89	2.52	—	–0.63
Uzbeks	29,141	19,927	24,499	26,888	3.55	2.81	1.86	2.84	+0.74	–0.98
Jews	11,903	12,953	19,758	21,510	1.45	1.65	1.50	1.77	–0.20	–0.27
Germans	—	998	18,572	18,822	—	0.71	1.41	0.84	—	+0.57
Kazakhs	—	11,956	17,123	20,166	—	1.77	1.30	1.82	—	–0.52
Poles	—	6,975	16,860	16,133	—	0.39	1.28	0.37	—	+0.91
Georgians	4,351	6,974	11,723	12,099	0.53	1.24	0.89	1.32	–0.71	–0.43
Armenians	5,089	6,975	11,064	10,755	0.62	1.22	0.84	1.26	–0.60	–0.42
Turkmen	—	4,982	9,352	9,411	—	0.46	0.71	0.46	—	+0.23
Latvians	—	1,191	4,742	5,400	—	0.04	0.58	0.07*	—	+0.51
Finns	—	997	2,371	2,750	—	0.09	0.29	0.08*	—	+0.21

*In some cases, and especially in those of Latvians and Lithuanians, the 1937 and the available 1939 data show notable discrepancies (see also Gerhard Simon, *Nationalismus und Nationalitätenpolitik in der Sowjetunion* [Cologne, 1986], 422–24). This inconsistency perhaps precludes refined analysis but does not prevent visualization of magnitude.

SOURCES: GARF (TsGAOR), fond 9414, opis' 1, delo 1155, listy 1, 11 (camp population: d. 1139, ll. 178–81; and d. 1140, ll. 191–92, give slightly different figures for January 1, 1938 and 1939); RGAE (TsGANKH), f. 1562, op. 329, d. 144 (1937 census data); Frank Lorimer, *The Population of the Soviet Union* (Geneva, 1946), 138–39 (1939 census data).

Germans and Poles being especially hard-hit. On the other hand, Ukrainians, Jews, Central Asians (except Turkmen⁴⁴) and people from the Caucasus were less represented in the GULAG system than in the population of the country; as national groups, they suffered proportionately less in the 1937–1938 terror.⁴⁵

If ethnic groups for whom camp figures are unavailable in 1937 were too weakly represented to be counted, then Table 4 accurately demonstrates the statistical impact of the terror on different nationalities. Because we know that the party/state administration was heavily staffed by Russians and that many members of the party elite and economic leadership were of Polish and German background, the changes in the ethnic composition seem to indicate a terror aimed

⁴⁴ We shall see that the case of the Turkmen can be explained by the particular cruelty of the purge in their republic.

⁴⁵ Ukrainians seem to have been more heavily repressed before 1934, when their share in the camp population had reached 19 percent. It is probable that a certain number of Ukrainian inmates were listed as Russians, Belorussians, or Poles. Data about the ethnic origin of executed people are unavailable at this writing, and these may modify the picture for the national background of the victims of the 1937–1938 terror but not that of previous and subsequent years, when many fewer persons were shot. Even after the occupation of the Western Ukraine, however, the share of Ukrainians in hard regime camps was 14.6 percent in 1940 and 12.6 percent in 1941 (versus 61 percent and 59 percent of Russians). Nevertheless, by 1951, the proportion of Ukrainians was 23.6 percent in the population of camps and that of Russians 52.6 percent, and 20 percent and 55.6 percent respectively in the combined population of camps and colonies. See GARF (TsGAOR), f.9414, op.1, d.1356, l.4.

more at the elite than at particular national groups per se.⁴⁶ To be sure, a sizable proportion of citizens of Polish and German origin living in border areas suffered several waves of "cleansing" for their alleged unreliability.⁴⁷ In addition, wherever they resided, they were likely to be accused of political sympathies with states with which relations were strained, especially at a time when the authorities suspected fifth columns throughout the country and ordered a clampdown on "spies and nationalists."⁴⁸ This circumstance must have contributed to the fact that, in early 1939, when GULAG inmates made up 0.77 percent of the country's population, some 2.7 percent and 1.3 percent of these ethnic groups were in hard regime camps, as well as about 1.3 percent of all Koreans, 1.7 percent of all Estonians, 1.9 percent of all Finns, and 3.2 percent of all Lithuanians, compared to approximately 0.85 percent of all Belorussians, 0.84 percent of all Russians, 0.65 percent of all Ukrainians, and 0.61 percent of all Jews. The national group suffering the most in proportional terms was the Latvians, who were heavily represented in the party and state administration and of whose total census population a staggering 3.7 percent was in strict regime camps alone.

The hypothesis of an increasingly anti-elite orientation of the penal policy is supported by data on the educational levels of labor camp inmates. Table 5 shows the educational background of hard regime camp inmates on January 1, 1937, alongside educational levels for the population as a whole in 1937. Even allowing for the rise in educational levels in the general population between 1937 and 1940, it seems clear that the purge hit those with higher educational levels more severely. Although less educated common folk heavily outnumbered the "intelligentsia" in the camps, those who had studied in institutions of higher or secondary education were proportionally nearly twice as numerous in the GULAG system as they were in society at large, while those with elementary (or no) education were under-represented.

Moreover, in the years spanning the Great Terror, the proportion of the camp population with some education rose significantly, while that of less educated people declined. From 1934 to 1941, the segment of the camp population with higher education tripled and the proportion with secondary education doubled. Again, however, care must be used in interpreting these data, because educational levels in the population as a whole were increasing steadily during the decade of

⁴⁶ The under-representation of those of Jewish background is somewhat surprising, given the relatively high proportion of Jews in the party membership and in responsible positions. At the beginning of 1937, they constituted the third largest ethnic group in the party, with 5.3 percent of all members. *Rossiiskii tsentr khraneniia i izucheniia dokumentov noveishei istorii*, hereafter, RTsKhIDNI, f.17, op.120, d.278, l.10. (This is the recently renamed Central Party Archive [TsPA], Institut Marksizma-Leninizma. We cite this collection below as RTsKhIDNI [TsPA]). It is possible, however, that in many cases the figures for the national composition of the camp population were based on the declarations of the inmates themselves and that a great number of Jewish communists felt sufficiently assimilated to identify with other ethnic groups.

⁴⁷ Political Archive of the Foreign Office, Bonn, Botschaft Moskau, A2 Innerpolitische Verhältnisse der Sowjetunion, vol. 8: the Leningrad Consulate General to the Embassy, June 30, July 20, and August 3, 1935, p.1; vol. 13: the Vladivostok Consulate to the Embassy, September 14, 1937; Botschaft Moskau, A4 Militär- und Marineangelegenheiten: the Leningrad Consulate General to the Embassy, May 28, 1935; Botschaft Moskau, A2I Kiew, Kurze Meldungen: the Kiev Consulate to the Embassy, May 27, 1936.

⁴⁸ Compare GARF (TsGAOR), f.8131sch, op.27, d.140, l.25; f.9401, op.1a, d.20, l.54; op.2, d.1, l.3.

Table 5. Educational Levels of the GULAG Population versus the USSR as a Whole, 1937

	<i>GULAG Population, 1937 (%)</i>	<i>USSR Population, 1937 (%)</i>
<i>School Achievement</i>		
higher	1.0	0.6
secondary	8.9	4.3
elementary	49.3	38.3*
semi-literate	32.4	—
illiterate	8.4	39.0

NOTE: *given as *gramotnye* (literate) in census

SOURCES: GARF (TsGAOR), fond 9414, opis' 1, delo 1155, list 10 (camp population); RGAE (TsGANKH), f. 1562, op. 329, d. 144, ll. 11–13 (1937 data).

the 1930s. We lack detailed annual education data for the period and especially statistics on the share of people with college and high school instruction in the population of the late 1930s and early 1940s. Thus it would be dangerous to draw firm conclusions, even though the available evidence strongly suggests that the terror intensified against the educated elite. It comprised 12.8 percent of the population of hard regime camps by 1941, compared to 6.3 percent in 1934. As Table 6 indicates, the number of detainees with higher and secondary education grew much faster than the rest of the GULAG population.

IT IS COMMONLY BELIEVED THAT MOST OF THE PRISONERS of the “Gulag Archipelago” had been arrested and sentenced for political offenses falling under one of the headings of “counterrevolutionary offenses” (Article 58 in the criminal code). It is also common wisdom that many people arrested for other reasons were accused of political crimes for propaganda value. The available evidence does not bear out this view, but it does suggest considerable ambiguity in definitions of “political crimes.” Table 7 shows the breakdown of labor camp inmates for selected years, according to the offense for which they were sentenced. Although the presence of alleged counterrevolutionaries is impressive, it turns out that ostensibly non-political detainees heavily outnumbered “politicals.”

In view of the murderous campaign of 1932–1933 against pilferers of state and

Table 6. Percentage of Increase in Detainees by Educational Background in GULAG Camps

	<i>1934–1936</i>	<i>1936–1939</i>	<i>1939–1941</i>
<i>Education</i>			
higher	+47.5	+69.6	+25.6
secondary	+54.1	+48.0	+23.5
elementary and less	+37.9	+34.4	+7.9

SOURCE: GARF (TsGAOR), fond 9414, opis' 1, delo 1155, list 10 (d. 1140, l. 190, gives slightly different figures for 1939).

**Table 7. Offenses of GULAG Population
(by Percent as of January 1 of each year)***

	1934	1936	1940
<i>Sentenced for:</i>			
"Counterrevolutionary offenses"	26.5	12.6	33.1
Dangerous crimes against the administrative order, including Banditry	15.2	17.7	3.6
Other crimes against the administrative order, including Speculation and "Hooliganism"	3.9 1.3 1.3 —	3.2 — 1.1 —	2.4 13.9 2.4 7.3
Misconduct in office, Economic crimes	7.5	10.6	7.3
Crimes against persons	4.7	5.5	5.2
Crimes against property	15.9	22.3	12.1
Theft of public property	18.3	14.2	1.9
"Socially harmful and dangerous elements"	8.0	11.5	18.9
Violation of the law on internal passports	—	2.3	1.3
Military offenses	0.6	0.8	0.7
Other delicts	2.0	2.6	3.3

NOTE: *The percentages do not add up to 100 because of rounding.

SOURCE: GARF (TsGAOR), fond 9414, opis' 1, delo 1155, listy 3–6.

collective farm property, and of the fact that in 1951 the number of prisoners convicted for this offense largely outstripped that of all categories of "counter-revolutionaries,"⁴⁹ their share seems at first glance suspiciously low in Table 7, especially in 1940. One explanation for the relatively low proportion of inmates convicted under the "Law of August 7, 1932"—which had prescribed the death penalty or ten years of hard labor for theft of state property—is an unpublished decree of January 1936 ordering the review of the cases of all inmates convicted under the terms of this Draconian law before 1935.⁵⁰ The overwhelming majority of these people had been condemned between 1932 and 1934, and four-fifths of this cohort saw their sentences reduced by August 1936 (including 40,789 people who were immediately released).⁵¹ Another possible explanation is that many people benefited from a directive reorienting the drive against major offenders and from reviews of their convictions that led by the end of 1933 to modifications of 50 percent of the verdicts from the previous seventeen months.⁵² This state of affairs seems to account for the considerable confusion in the records concerning the implementation of the "Law of August 7" and for the fact that, while claiming that the number of persons sentenced under its terms was between 100,000 and 180,000, officials were reluctant to advance exact figures even as late as the spring of 1936.⁵³

⁴⁹ That is, 709,348 detainees—28 percent of all camp and colony inmates—versus 579,918—22.9 percent; GARF (TsGAOR), f.9414, op.1, d.1356, ll.1–3.

⁵⁰ GARF (TsGAOR), f.3316, op.2, d.1754, ll.2–3.

⁵¹ GARF (TsGAOR), f.3316, op.2, d.1837, ll.88–89.

⁵² *Sovetskaiia iustitsiia*, no. 24 (1934): 2–3; *Sotsialisticheskaia zakonnost'*, no. 8 (1937): 38; *Ugolovnyi kodeks* (1937): 131–32.

⁵³ See, for example, GARF (TsGAOR), f.3316, op.2, d.1534, ll.87, 112; d.1754, ll.21, 26; f.9474, op.16, d.48, ll.15, 17, 35–36, 42; d.79, ll.6, 16. In January 1933, even the people's commissar of justice, N. V. Krylenko, had no exact idea how many people had been sentenced to death and how many of them were in fact shot under the terms of the decree (compare GARF [TsGAOR], f.9474,

The category of “socially harmful and dangerous elements” and the manner it was put to use must also warn us not to accept the definitions of “counterrevolutionaries” in our sources. Article 7 of the penal code stated that “to persons having committed socially dangerous acts or representing danger through their relation[s] with the criminal milieu or through their past activities, measures of social defense of a judicial-corrective, medical or medico-pedagogical character are applied.” Nevertheless, it failed to specify penalties except to indicate in Article 35 that these persons could be subjected to internal exile, without giving the slightest hint of the sentences courts were entitled to pass.⁵⁴ The definition of the offense and the corresponding penalty were more than vague, but this did not prevent extra-judicial bodies of the secret police from singling out “harmful” and “dangerous” people among “recidivists [and] persons associated with the criminal milieu conducting a parasitic way of life etc.”⁵⁵ This information comes from an appeal to the top leadership by the procurator general, who was proposing to restrict the sentencing powers of the NKVD Special Board at the beginning of 1936 but not insofar as “dangerous elements” were concerned.

Although the procurator of the USSR, Andrei Vyshinskii, valued procedural precision, his office does not appear to have objected to the launching in August 1937 of a lethal “mass operation” targeting “criminals (bandits, robbers, recidivist thieves, professional smugglers, recidivist swindlers, cattle thieves) engaged in criminal activities and associated with the criminal milieu”—whether or not they were actually guilty of any specific offense at the moment—and connecting these common criminals to a wide range of supposedly “anti-Soviet” and “counter-revolutionary” groups, from “kulaks” to former members of forbidden political parties, former oppositionists, and alleged terrorists.⁵⁶ Clearly, the regime saw a political threat in the conduct, and indeed in the sheer existence, of “dangerous” persons. The secret directive of 1937 was no dead letter: the records suggest that it led to the arrest of a great number of people, some of whom were hardly more than notorious hooligans and yet were sometimes sent to the firing squad.⁵⁷

Some 103,513 “socially harmful and dangerous elements” were held in hard regime camps as of January 1937, and the number grew to 285,831 in early 1939, when, as Table 3 shows, they made up a record 21.7 percent of all detainees (and 56.9 percent of juvenile detainees). But the proportion (and also the number) of “dangerous” persons began to decline by January 1940 and that of “hooligans” started to rise, until the size of their contingent came close to that of the “harmful

op.1, d.76, l.118; and V. P. Danilov and N. A. Ivinskii, “O derevne nakanune i v khode sploshnoi kollektivizatsii,” in Danilov and Ivinskii, eds., *Dokumenty svidetel'stvuiut* [Moscow, 1989], 41–42).

⁵⁴ Although an addendum in 1930 forbade the exile of juveniles below 16 years of age, the widespread practice of deporting “kulak” families made short shrift of it. A 1946 decision of the Supreme Court explained that “socially dangerous elements” could be sent to exile “also in the case when they would be acquitted by the court for the accusation of having committed a specific crime”; *Ugolovnyi kodeks RSFSR* (Moscow, 1956), 138.

⁵⁵ GARF (TsGAOR), f.8131sch, op.27, d.70, l.103.

⁵⁶ *Trud* (June 4, 1992): 4.

⁵⁷ See, for example, GARF (TsGAOR), f.7523, op.65, d.557, ll.29–30, 42–45, 49, 53. In some cases, the sentence was reviewed after the spring of 1938, which led to the release of a certain number of people.

elements" by 1941, in part because of toughened legislation concerning rowdies.⁵⁸ A total of 108,357 persons were sentenced in 1939 for "hooliganism"; in the course of the next year, 199,813 convicts fell into this category. But by 1948, the proportion of "hooligans" among camp inmates was 2.1 percent, whereas that of "dangerous elements" fell to 0.1 percent.⁵⁹ No doubt the same offense in the 1930s could be regarded as "socially dangerous" and in the 1940s as "hooliganism."⁶⁰

"Socially harmful" people may have been victims of political repression, but it would be far-fetched to presume that the unjust punishment they received was a response to conscious acts of opposition to the regime. Having observed this, we must remember that the great majority of those sentenced for "counterrevolutionary offenses" had never committed any act deliberately directed against the Soviet system and even continued to remain faithful to the Bolshevik cause, notwithstanding their victimization. From this point of view, the regime's distinction between "political" and "non-political" offenders is of doubtful relevance. Unless we are prepared to accept broad Stalinist definitions of "counterrevolutionary" offenses or the equally tendentious Western categorization of *all* arrests during Stalin's time (even those for crimes punishable in any society) as political, we should devise ways to separate ordinary criminality from genuine opposition to the system as well as from other reasons for which people were subjected to penal repression.

At any rate, the Appendix figures show that from 1934 to 1953, a *minority* of the labor camp inmates had been formally convicted of "counterrevolutionary crimes." Our data on sentencing policy are incomplete for the period before 1937, but they permit us to advance some estimates of orders of magnitude. Thus we can calculate that only about 11 percent of the more than 5.3 million persons sentenced by courts and extra-judicial bodies between 1933 and 1935 represented "cases of the OGPU/NKVD"⁶¹ of which, as we have seen, a relatively high proportion had not been considered "political." Some 28 percent of the almost 5 million people convicted by various courts and NKVD boards in 1937–1939 were sentenced "from cases of the security police," mostly under the pretext of "counterrevolutionary offenses." But while the judiciary and the Special Board of the NKVD/MVD subjected nearly 31 million persons to penalties in the period 1940–1952, only 4.8 percent (though a sizable 1.5 million persons) fell under Article 58. By contrast, more than twice as many (11 percent) of all people sentenced in these years were charged with appropriating public property.⁶²

It turns out that by far the largest group of those sentenced between 1940 and 1952 consisted of people accused of violating laws devised to strengthen labor

⁵⁸ GARF (TsGAOR), f.9414, op.1, d.1155, l.5; *Ugolovnyi kodeks RSFSR* (Moscow, 1947), 154; *Ugolovno-protsessual'nyi kodeks RSFSR* (Moscow, 1947), 196–97.

⁵⁹ GARF (TsGAOR), f.9414, op.1, d.1155, ll.5–6; f.9492, op.6, d.14, l.14.

⁶⁰ See the injunction to courts to clamp down on "hooligan misbehavior of a counterrevolutionary character" in *Sovetskaia iustitsiia*, no. 18 (1935): 10.

⁶¹ Calculated on the basis of GARF (TsGAOR), f.9401, op.1, d.4157, l.203; and f.9474, op.1, d.97, l.59. Having combined court statistics with police data referring also to certain persons condemned by the judiciary, we must concede that it is possible a small number of them figure twice in our computation.

⁶² Compared to some 9 percent in 1937–1939.

Table 8. GULAG Population according to Sentencing Authority
(Percentages as of January 1)*

	1934	1935	1936	1937	1938	1939	1940	1941
<i>Jurisdiction</i>								
Police bodies	42.2	41.3	33.7	30.9	49.8	59.4	54.5	38.7
Including:								
the Special Board of the NKVD					3.7	8.3	9.4	8.2
the "Special Troikas" of 1937-1938						23.3	25.4†	17.2
Courts and Tribunals	57.8	58.7	66.3	69.1	50.2	40.6	45.5	58.6

NOTES:

*There was no corresponding information on some 2.6 percent of the detainees for 1941.

†The increase of this cohort, despite the abolition of this jurisdiction in November 1938, was no doubt due to the transfer of inmates who had been in colonies before the end of 1939.

SOURCE: GARF (TsGAOR), fond 9414, opis' 1, delo 1155, list 8 (see also f. 8131sch, op. 27, d. 70, l. 141, where similar though not entirely identical data can be found for 1934-1935).

discipline, ranging from unauthorized absence from work to dodging mobilization for work in agriculture, to failing to meet the compulsory minimum of work in the collective farm. Although the judiciary jargon called them "wartime decrees," most of them remained in force until 1956. More than 17 million people had been convicted under their terms between 1940 and 1952 (albeit "only" 3.9 million of them were sentenced to detention), comprising half (55.3 percent) of all the period's sentences.⁶³ One may wonder if acts infringing on proprietary prerogatives and labor relations in a state that is virtually the only proprietor and practically the only employer do not bear some relation to politics. But if we leave aside this dilemma as well as the year 1936, for which our data are too fragmentary, we can conclude that, on the whole, only about 8.4 percent of the sentences of courts and extra-judicial bodies were rendered "on cases of the secret police" and for alleged political reasons between 1933 and 1953.

From 1934, when many believe the terror was mounting, to 1937-1938, the camp proportion of "counterrevolutionaries" actually declined. Table 8 shows that so did the proportion in the strict regime camp population of those who had been sent there by specific police bodies.

Even though the number of people convicted "on cases of the NKVD" more than tripled from 1934 to 1935; a careful look at the sources shows that many sentences had hardly anything to do with "political" cases. Data on the arrested "counterrevolutionaries" show a 17 percent growth due to an increase in the number of people accused of "anti-Soviet agitation" by a factor of 2.6.⁶⁴ As for

⁶³ GARF (TsGAOR), f.9401, op.1, d.4157, ll.201-02; f.9492, op.6, d.14, ll.6-8, 10, 14; d.15, ll.12-13. To avoid double-counting, we used NKVD figures for the number of "politicals" sentenced in 1937-1938, which are higher than those of the judicial statistics. Our data for 1940-1952 are on "counterrevolutionaries." As for the numbers of pilferers and violators of labor discipline, they certainly include the same persons more than once in some cases, since these offenses were likely to be committed repeatedly. For some legal dispositions that account for the high number of custodial sentences under the "wartime decrees," see *Ugolovnyi kodeks RSFSR—Kommentarii*, 282, 284-85.

⁶⁴ GARF (TsGAOR), f.9401, op.1, d.4157, l.203.

sentences in 1935, 44.6 percent of them were rendered by regional NKVD "troikas" (tribunals), which did not deal with "political" affairs.⁶⁵ Another 43 percent were passed by regular courts, but fewer than 35,000 of the more than 118,000 people concerned had been "counterrevolutionaries."⁶⁶ To be sure, the quantity of "political" sentences increased, compared to the previous year. In 1936, however, the NKVD arrested the same number of "counterrevolutionaries" as in 1934, which does not seem to show steadily intensifying political repression. Similarly, the continually decreasing number of people shot in cases initiated by the secret police⁶⁷ and the constantly diminishing share (as well as aggregate number) of "counterrevolutionaries" in hard regime camps between 1934 and 1937 casts doubt on the idea of "mounting" repression in this period.

The abolition of the OGPU, a degree of uncertainty concerning the sentencing privileges of the new NKVD, and attempts to transfer the bulk of "political" cases to the jurisdiction of military tribunals as well as to the special boards of regional courts and the Supreme Court⁶⁸ suggest that the penal policy of more or less ordinary judicial instances, whose statistics are available, is indicative of the general trend of 1935–1936. The data are unfortunately incomplete, but we have information on at least 30,174 "counterrevolutionaries" who were sentenced by civilian and military courts in 1935, in the wake of the Kirov assassination, and on 19,080 people who were prosecuted by the same courts for supposedly political offenses in the first half of the next year.⁶⁹ Most of this growth is attributable to the increased frequency of "anti-Soviet agitation," which accounted for 46.8 percent of the cases before the courts of the Russian Federation in the first six months of 1935, and 71.9 percent in the corresponding period of the next year.⁷⁰ The loose application of this charge did not always sit well in high places, and the people's commissar of justice along with the prosecutor general warned top decision-makers of the consequences of an excessive use of the more than vague legislation on "counterrevolutionary agitation."⁷¹ The prosecutor general had a heated exchange of letters with the head of the security police that raised the possibility of limiting NKVD jurisdiction in this matter.⁷²

There was a tendency to *diminish* rather than inflate the share of "political" cases in 1936. Even the chairman of the ominous Military Collegium of the Supreme Court noted in December 1936 that the number of "counterrevolutionaries" convicted by his bench and its subordinate courts in the first nine months of the year was 34.4 percent *less* than in the same period of 1935. The number of prosecutions had grown only for two categories of crimes. Characteristically

⁶⁵ GARF (TsGAOR), f.8131sch, op.28, d.6, l.62.

⁶⁶ GARF (TsGAOR), f.9474, op.1, d.97, l.21.

⁶⁷ Some 2,056 such executions are on record in 1934 versus 1,229 in 1935 and 1,118 in 1936; GARF (TsGAOR), f.9401, op.1, d.4157, l.203.

⁶⁸ GARF (TsGAOR), f.8131sch, op.27, d.73, l.228; f.9474, op.1, d.85, l.7; *Sovetskaiia iustitsiia*, no. 19 (1934): 4; *Sobranie zakonov i rasporiazhenii* . . . (1935): 139–40.

⁶⁹ GARF (TsGAOR), f.9474, op.1, d.97, l.21; d.104, ll.123, 133, 146.

⁷⁰ GARF (TsGAOR), f.8131sch, op.27, d.71, l.127; f.9474, op.1, d.104, ll.123, 126, 130; op.16, d.97, l.113. The crime did not seem to have entailed the hardest penalties at this time, since about half the convicts were sentenced to terms of between two and five years.

⁷¹ GARF (TsGAOR), f.8131sch, op.27, d.71, ll.127–33; d.73, ll.228–34.

⁷² GARF (TsGAOR), f.8131sch, op.27, d.70, ll.103–06, 134–36, 138–42.

enough, these were espionage and sabotage, and their frequency increased, especially in the third quarter of 1936.⁷³

It is from that time, late 1936, and not from late 1934 that the number of "counterrevolutionaries" (as well as the cohort sentenced by the NKVD) began to swell dramatically, above all in the wake of the launching of wholesale "mass operations" during the summer of 1937 that victimized "socially harmful" people alongside a wide range of purported political delinquents. The documents that ordered the mass "repression of former kulaks, criminals, and anti-Soviet elements" through decisions of newly organized "Special Troikas" of the secret police specified that the operation had to be completed within four months and even set "control figures" for the numbers of people to be shot and imprisoned. The relevant instruction foresaw 72,950 executions and 186,500 new detainees as the outcome of the drive and stipulated that the numerical targets were not to be exceeded without authorization of the Moscow headquarters of the NKVD.⁷⁴

Nothing indicates that the operation enjoyed a more orderly implementation than any other campaign in the Soviet system of planning. Available documentation on the course of the action is fragmentary, but it shows that after mid-February 1938, when according to the initial orders the operation should have been over for more than two months, the chief of the NKVD requested additional funding for the detention and transportation of about twice the number of people spoken about in the original directives.⁷⁵ Moreover, the "Special Troikas" had largely "overfulfilled plans" by this time, having doomed 688,000 people before the end of 1937. Similarly, the expectations of the NKVD boss proved equally low compared to the 413,433 persons actually subjected to the jurisdiction of the local "troikas" in 1938.⁷⁶ Local enthusiasm outstripped the expectations of the center.

In general, the leadership of the terror was not very good at predicting events. In December of 1936, NKVD chief N. I. Ezhov issued a secret order to the effect that the number of inmates at SEVVOSTLAG (Kolyma) should be 70,000 in 1937 and 1938.⁷⁷ (This was its population as of July 1936.) But this "plan" was overfulfilled by 20,000 in the second half of 1937, and by the end of 1938 the camp housed 138,170, twice the planned level.⁷⁸ Characteristically, as late as February 1938, the GULAG administration was at a loss to give the exact number of victims falling under its authority nationally.

Some local camp commandants found the numbers of convicts modest by the early months of 1938 and bombarded Moscow with telegrams asking for a larger

⁷³ GARF (TsGAOR), f.9474, op.1, d.104, ll.144, 146. For other documents suggesting that in 1936 the prevailing line was not to find "enemies" at all cost, see d.86, ll.85, 91; d.97, l.17; d.99, ll.10–11, 91; f.8131sch, op.27, d.62, ll.62, 69, 78–81; d.70, ll.103–06, 134–36, 138–42; f.9492, op.1s, d.1, l.1.

⁷⁴ *Trud* (June 4, 1992): 4. It also provided a breakdown by republics. There are few reasons to doubt the authenticity of these documents, since some of the measures they enumerate reappear in a source we have consulted; GARF (TsGAOR), f.5446, op.57, d.52, l.26. For a sudden increase in the number of people among "kulak" exiles listed as "sentenced" in 1937–1938, see Zemskov, "Spetsposelentsy," 6.

⁷⁵ GARF (TsGAOR), f.9414, op.1, d.15, ll.59–60, 192.

⁷⁶ GARF (TsGAOR), f.9401, op.1, d.4157, l.202; f.9414, op.1, d.1138, l.20. It is highly probable that our sources indicate only an approximate figure for 1937 "troika" victims.

⁷⁷ GARF (TsGAOR), f.9401, op.1a, d.9, l.341.

⁷⁸ GARF (TsGAOR), f.9414, op.1, d.1155, l.20.

"labor force,"⁷⁹ probably because their production plans were calculated on the basis of larger contingents than the ones at their disposal. Still, hundreds of thousands of new inmates arrived after the summer of 1937 to camps unprepared to accommodate them. At the moment when the head of the secret police was applying for an increase in the NKVD budget to receive a new influx of prisoners, reports of the procurator general—who was supposed to supervise penal institutions—painted a dreary picture of the lack of elementary conditions of survival in the GULAG system as well as of starvation, epidemic disease, and a high death rate among those already there.⁸⁰ The year 1938 saw the second highest mortality in hard regime camps before the war and probably also in prisons and labor colonies, where 36,039 deaths were recorded, compared to 8,123 in 1937 and 5,884 in 1936.⁸¹

Returning to the question of plan and control over the purge, we find a letter in which the NKVD chief promised to improve the poor camp conditions, yet he reported figures for the increase in GULAG population different from the data reported by his own administration.⁸² Evidence also suggests that the NKVD and the Central Committee issued directives during the drive that were incompatible with each other.⁸³ In addition, there is at least one republic on record, that of Belorussia, where vigilant local officials continued mass shootings for a time even after an order was dispatched calling for an end to the wholesale purge.⁸⁴

Although the theoretical capacity of the prisons in Turkmenistan was put at 1,844 places, 6,796 people had been locked up in them at the beginning of 1938, and 11,538 by May; this was clearly unanticipated in Moscow.⁸⁵ The dimensions the campaign reached in the republic explains the over-representation of Turkmen among camp inmates. Other ethnic groups also suffered—at one time, all of Ashkhabad's 45 Greek residents were arrested as members of an "insurrectionary organization."⁸⁶ The NKVD chief of the republic prescribed "control figures for cases of espionage [and] sabotage" as well as specific "limits" for the number of arrests to celebrate May Day,⁸⁷ which suggests that after a while, the operation was farmed out to regional heads of the secret police. A fire at a factory became an occasion to meet "quotas" for sabotage by arresting everybody who happened to be there and forcing them to name their "accomplices" (whose number soon

⁷⁹ GARF (TsGAOR), f.9414, op.1, d.1139, ll.118–22.

⁸⁰ GARF (TsGAOR), f.8131sch, op.27, d.111, ll.5–6, 34.

⁸¹ About 6.9 percent of the yearly average population of GULAG camps perished in 1938 and 15.2 percent in the famine year of 1933; GARF (TsGAOR), f.9414, op.1, d.1155, ll.1–2; d.2740, ll.52–53.

⁸² GARF (TsGAOR), f.9414, op.1, d.15, l.190; d.1138, ll.6, 70. It is unlikely that Ezhov's intention was to conceal the real figure, because the number he furnished was within the same range as the two other series of data we have on new arrivals. By the way, they were compiled at different times and are not identical.

⁸³ GARF (TsGAOR), f.8131sch, op.27, d.145, ll.109–10, 125.

⁸⁴ GARF (TsGAOR), f.8131sch, op.27, d.118, ll.74–78.

⁸⁵ GARF (TsGAOR), f.9414, op.1, d.1138, ll.122–23. Some 148 people died in Turkmenistan's jails and labor colonies in the course of May 1938 alone.

⁸⁶ GARF (TsGAOR), f.8131sch, op.27, d.145, ll.72–73.

⁸⁷ GARF (TsGAOR), f.8131sch, op.27, d.145, ll.53, 57.

exceeded one hundred persons).⁸⁸ If nothing else worked, it was always possible to round up people having the bad luck to be at the marketplace, where a beard made one suspect of the "crime" of being a mullah and where more than 1,200 "counterrevolutionaries" were seized in a matter of five months.⁸⁹ Mock executions and incredibly savage torture were used in Turkmenistan to wring out confessions to all sorts of "subversive acts" and "organizations."⁹⁰ To be sure, neither torture nor trumped-up cases was a Turkmen monopoly: the records show that both became widespread in the wake of the wholesale purge the "Special Troikas" spearheaded.⁹¹

This state of affairs illustrates the problems posed by our sources on the question of "politicals." A person arrested for his "suspicious" Polish origin or shot because of having been married to a Pole in the past was no doubt accused of being a "counterrevolutionary."⁹² We can also only wonder how many victims shared the fate of namesakes and were sentenced to long terms or shot as alleged former members of defunct parties.⁹³ How many people were like the peasant who had been condemned "merely" to ten years but whose paperwork slipped in among that of people slated for capital punishment? (He was shot with them.)⁹⁴ Probably, most such people figure in our data on "politicals," even if some of the mistakenly executed were listed under the heading of their original "non-political" sentences.

Last but not least, there was the purge of the purgers: how "counterrevolutionary" were the great number of officials of the NKVD and the judiciary who were denounced for "anti-Soviet activities" after November 1938, when the Central Committee abolished the "troikas," called off the purge, and decided that "enemies of the people and spies having made their way" into the secret police and the procuracy had been responsible for the terror of the preceding period?⁹⁵ Many of these "hostile elements" were sentenced as "politicals," just as the majority of those they had cruelly mistreated, although they continued to protest their fidelity to the regime until the very end.⁹⁶

But whatever we think about "counterrevolutionaries," their identified cohort constituted 34.5 percent of the camp population by 1939. This was not their largest share in the pre-war period: at the beginning of 1932, people sentenced for "political" reasons in what corresponded then to hard regime camps comprised 49 percent of the inmates.⁹⁷ The widespread recourse to capital punish-

⁸⁸ GARF (TsGAOR), f.8131sch, op.27, d.145, ll.73-74.

⁸⁹ GARF (TsGAOR), f.8131sch, op.27, d.145, ll.46, 56-57.

⁹⁰ GARF (TsGAOR), f.8131sch, op.27, d.145, ll.46-47, 52-53, 58-60, 62, 67-70.

⁹¹ See, for example, GARF (TsGAOR), f.8131sch, op.27, d.145, ll.24, 190-91; d.118, ll.19-20, 25-26, 32-33, 35-36, 57-59; d.139, ll.26, 36-40, 42, 95, 119; d.140, ll.24-25; d.240, ll.172, 249-50; d.244, ll.19-20.

⁹² GARF (TsGAOR), f.7523, op.65, d.567, l.23; f.8131sch, op.27, d.145, ll.76-77.

⁹³ GARF (TsGAOR), f.8131sch, op.27, d.145, l.65.

⁹⁴ GARF (TsGAOR), f.8131sch, op.27, d.139, l.12.

⁹⁵ RTsKhIDNI (TsPA), f.17, op.3, d.1003, ll.85-86; GARF (TsGAOR), f.8131sch, op.27, d.118, l.32; d.140, l.25; d.145, ll.50, 101; d.239, l.45.

⁹⁶ RTsKhIDNI (TsPA), f.17, op.3, d.1003, l.84; GARF (TsGAOR), f.7523, op.65, d.568, ll.49-52, 60-66; f.8131sch, op.27, d.240, ll.173-74.

⁹⁷ "Svedenie o sostave zakliuchennykh, soderzhaiushchikhsia v ispravit.-trudovykh lageriakh NKVD," 1, 2. (We are again grateful to the society Memorial for putting documents at our disposal.)

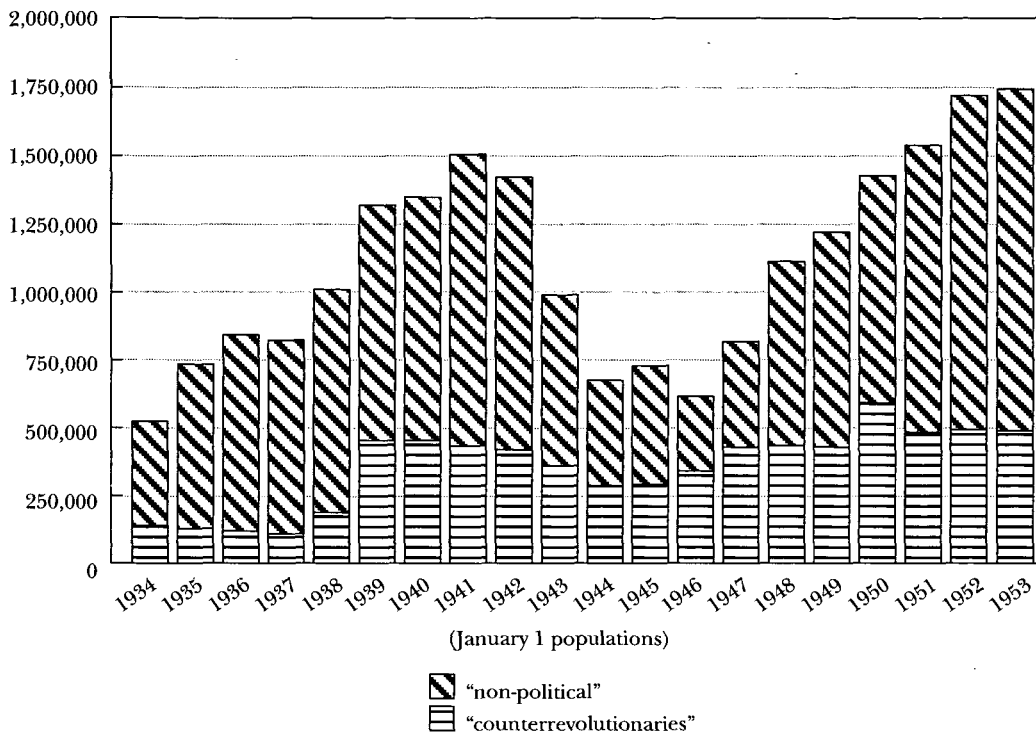


FIGURE B: "Political" Crimes as Proportion of GULAG Population, 1934–1953

ment in 1937–1938 is responsible for holding the proportion of "counterrevolutionaries" under 50 percent until 1946. The percentage then declined again, probably as the result of a renewed offensive against pilferers of public property.⁹⁸ If we superimpose the numbers of purportedly political inmates on the oscillating population of the labor camps from year to year, we find that while the proportion of "counterrevolutionaries" fluctuated, their aggregate numbers remained remarkably constant from 1939 until Stalin's death (Figure B). This suggests that, numerically, a cohort of "politicals" was taken into the camps at the time of the Great Terror and remained relatively constant in future years.

THE TIME OF THE GREAT PURGES (1936–1939), as Figure C indicates, was numerically not the period of greatest repression, even if we take into account the masses of people shot in 1937–1938 and the much less frequent recourse to capital punishment from the late 1940s. Annual numbers of detainees were

⁹⁸ Rittersporn, *Stalinist Simplifications and Soviet Complications*, 273–74. The steep rise of the share of prisoners listed under the heading of "counterrevolutionaries," from 41.2 percent in 1945 to 59.2 percent by January 1, 1946, was in part due to the amnesty of July 1945 that freed a large number of detainees and was not applicable to "politicals"; *Sbornik dokumentov po istorii ugolovnogo zakonodatel'stva SSSR i RSFSR, 1917–1952 gg.* (Moscow, 1953), 426–27. It seems that their share in the combined population of camps and labor colonies was 36.4 percent or, according to another source, 34.1 percent on January 1, 1947, and 25.8 percent at the beginning of 1948; Dugin, "Neizvestnyi Gulag," 42, 49.

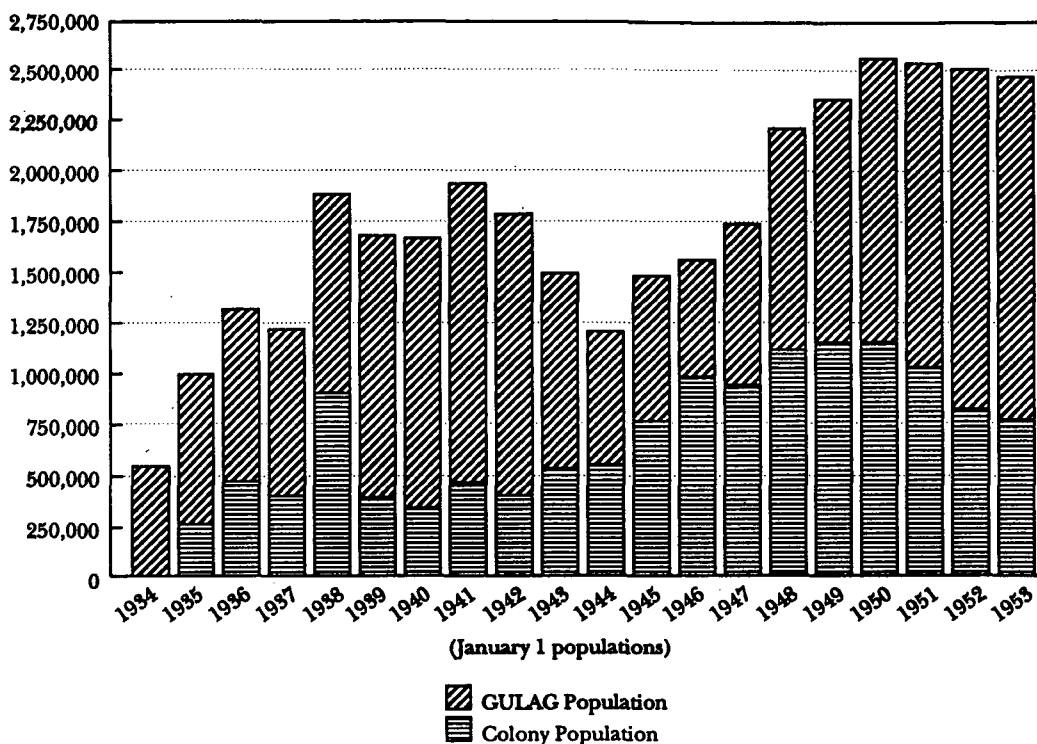


FIGURE C: GULAG and Colony Populations, 1934–1953

greater after World War II, reaching a peak shortly before Stalin's death.⁹⁹ If we extract the war years from the trend, we find that the picture is one of steadily increasing repression throughout the 1930s and 1940s.

Looking specifically at the hard regime camp populations (Figure C and the Appendixes), we find that in the twenty years from 1934 through 1953, the annual population increased in fourteen of the years and dropped in six. Of the six declining years, four were wartime; we know that approximately 975,000 GULAG inmates (and probably also a large number of persons from labor colonies) were released to military service.¹⁰⁰ Nevertheless, the war years were not good ones for the GULAG. First, many of those released to the army were assigned to punitive or "storm" formations, which suffered the heaviest casualties. Second, at the beginning of the war, prominent political prisoners were transferred and isolated in the most remote and severe camps in the system and most "politicals" were specifically barred from release to the military. Third, of the 141,527 detainees who had been in jails and evacuated during the first months of the war from territories soon to be occupied by the enemy, 11,260 were

⁹⁹ The unprecedented growth of the camp population after early 1947 was less a result of the increasing prosecution of "counterrevolutionaries" than of the imprisonment of other categories of offenders and a general rise from 1947 of the average length of sentence for a number of offenses having little to do with Article 58; GARF (TsGAOR), f.9492, op.6, d.14, ll.29–31.

¹⁰⁰ GARF (TsGAOR), f.9414, op.1, d.68, l.8.

executed.¹⁰¹ Fourth, in the first three years of the war, 10,858 inmates of the GULAG camps were shot, ostensibly for being organizers of underground camp organizations.¹⁰²

Finally, wartime life became harder for the remaining camp residents. More than half of all GULAG deaths in the entire 1934–1953 period occurred in 1941–1943, mostly from malnutrition. The space allotment per inmate in 1942 was only one square meter per person, and work norms were increased.¹⁰³ Although rations were augmented in 1944 and inmates given reduced sentences for overfilling their work quotas, the calorie content of their daily provision was still 30 percent less than in the pre-war period.¹⁰⁴ Obviously, the greatest privation, hunger, and number of deaths among GULAG inmates, as for the general Soviet population, occurred during the war.

The other years of significant population decrease in the camps were 1936 and 1953–1954. In 1936, the number of persons in both the GULAG system and labor colonies declined, as did the proportion of those incarcerated for “counterrevolution” and on sentences of the NKVD. Similarly, while the aggregate numbers of detainees were generally increasing between 1934 and 1937, the rate of increase was falling. In 1953, the year that saw the deaths of both Stalin and his secret police chief L. P. Beria, more than half of the GULAG inmates were freed.

We have fairly detailed data about the internal movement of persons—arrivals, transfers, deaths, and escapes—inside the strict regime camp network (see the Appendixes and Figure D). They confirm Solzhenitsyn’s metaphor that this was a universe in “perpetual motion.” Large numbers of persons were constantly entering and leaving the system. During the 1934–1953 period, in any given year, 20–40 percent of the inmates were released, many times more than died in the same year. Even in the terrible year of 1937, 44.4 percent of the GULAG labor camp population on January 1 was freed during the course of the year.¹⁰⁵ Until 1938–1939, there were also significant numbers of escapes from the hard regime camps. In any year before 1938, more of the GULAG inmates fled the camps than died there. A total of about 45,000 fugitives were on record in the spring of 1934,¹⁰⁶ a year when a record number of 83,000 detainees took flight. Between 1934 and 1953, 378,375 persons escaped from the GULAG camps.¹⁰⁷ Of them, 233,823 were recaptured, and the remaining 38 percent made good their escape.

¹⁰¹ Dugin, “Neizvestnyi Gulag,” 29–30. It is specified that 9,817 of them were shot in the prisons, 674 allegedly for “revolt[s] and resistance” while in transit, and 769 “illegally,” also while being transported.

¹⁰² GARF (TsGAOR), f.9414, op.1, d.68, ll.8–10.

¹⁰³ GARF (TsGAOR), f.9414, op.1, d.68, l.18.

¹⁰⁴ GARF (TsGAOR), f.9414, op.1, d.68, l.21.

¹⁰⁵ Some 53,778 inmates were released from the labor camps in the first quarter of 1940; 66.5 percent of them had served their full sentences. Another 30.6 percent had seen their sentences reduced or quashed; GARF (TsGAOR), f.9414, op.1, d.1155, l.28.

¹⁰⁶ GARF (TsGAOR), f.8131sch, op.27, d.28, l.32.

¹⁰⁷ We lack comparable data for labor colonies. One of the few available sources relates to a colony in the Smolensk area with 431 inmates in the spring of 1934, of whom 193 were condemned to detention and 238 to corrective labor and from which 507 persons had escaped in the last three months of 1933—including 156 people serving prison terms—and 433 in the first quarter of 1934, of whom 188 had been sentenced to confinement. “Smolensk Archive,” WKP 351, 52, 55. For indications that this colony was not an exception, see GARF (TsGAOR), f.8131, op.11, d.106, l.73; d.109, ll.120, 125; op.28, d.5, l.2.

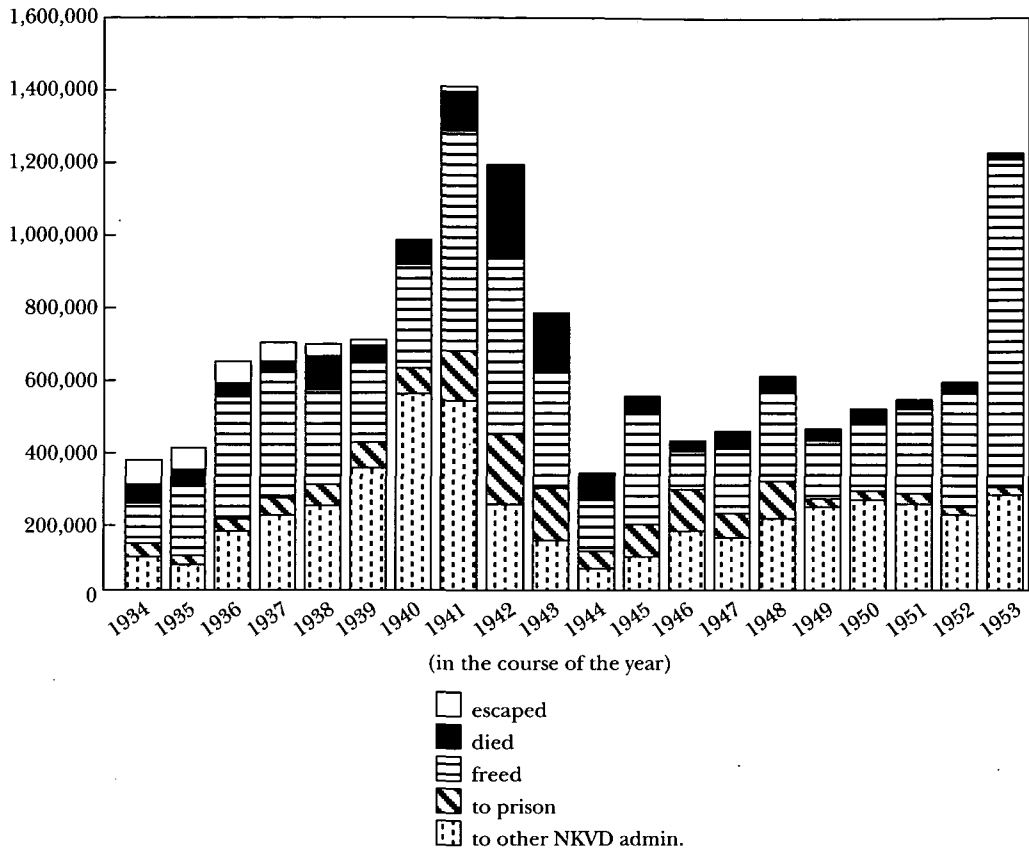


FIGURE D: GULAG Population Shifts, 1934–1953

The data show, however, that the number of escapes fell sharply beginning in 1938, as Stalin with Ezhov and then with Beria tightened camp regimes and security.¹⁰⁸

The data also indicate that the average length of sentence increased in the last years before the war. The longer terms “counterrevolutionaries” were likely to receive must have contributed to the growth of the proportion of people serving more than five years. However, Table 9 suggests that—despite a notable drop in the share of long terms meted out by the courts—the sentencing policy for inmates of hard regime camps came closer by the late 1930s to the one applied to “politicals” around mid-decade.

Even if most camp convicts were “non-political,” were only serving sentences of up to five years, and hundreds of thousands were released every year, the GULAG camps were horrible places. Work was hard, rations were barely adequate, and living conditions were harsh. The inmates were exposed to the exactions of fellow prisoners and especially to the cruelty of the guards.¹⁰⁹ Behind our figures lies the suffering of millions of people.

¹⁰⁸ Volkogonov, *Triumf i tragediia*, vol. 1, part 1, 43; RTsKhIDNI (TsPA), f.17, op.2, d.577, l.9.

¹⁰⁹ See, for example, GARF (TsGAOR), f.8131sch, op.27, d.28, ll.29–31.

Table 9. Length of Sentences during Stalinist Repression, 1935–1940 (by percent)

	RSFSR courts for common crimes, first half of:		USSR civilian courts for political crimes, first quarter of:	USSR courts	in GULAG camps, January
	1935	1936	1936	1939	1940
<i>Length</i>					
10+ years	—	—	—	0.1	1.0
5–10 years	20.0	17.6	50.7	4.0	42.2
up to 5 years	80.0	82.4	44.2	95.9	56.8

NOTE: The data on penalties concerning common crimes and for 1939 summarize only custodial sentences. Detention for more than 10 years was introduced in October 1937.

SOURCE: GARF (TsGAOR), fond 9474, opis' 1, delo 104, listy 8, 126 (1935 and 1936 data); f. 9492, op. 6, d. 14, l. 29 (1939 data); f. 9414, op. 1, d. 1155, l. 7 (camp population).

THE LONG-AWAITED ARCHIVAL EVIDENCE ON REPRESSION in the period of the Great Purges shows that levels of arrests, political prisoners, executions, and general camp populations tend to confirm the orders of magnitude indicated by those labeled as "revisionists" and mocked by those proposing high estimates.¹¹⁰ Some suspicions about the nature of the terror cannot be sustained, others can now be confirmed. Thus inferences that the terror fell particularly hard on non-Russian nationalities¹¹¹ are not borne out by the camp population data from the 1930s. The frequent assertion that most of the camp prisoners were "political" also seems not to be true. On the other hand, the new evidence can support the view, reached previously by statistical study and evidence of other types, that the terror was aimed at the Soviet elite.¹¹² It also confirms the conclusions of authors who had studied the available sources and shown the uncertainties of legal theory and penal practice in the 1930s.¹¹³ In addition, it seems that much of the process was characterized by high-level confusion and by local actions in excess of central plans.

The Stalinist penal system can be profitably studied with the same sociological tools we use to analyze penal structures elsewhere. It contained large numbers of common criminals serving relatively short sentences, many of whom were released each year and replaced by newly convicted persons. It included a wide variety of sanctions, including non-custodial ones. For most of those drawn into it,

¹¹⁰ See Hough and Fainsod, *How the Soviet Union Is Governed*, 177; and S. G. Wheatcroft, "More Light on the Scale of Repression and Excess Mortality in the Soviet Union in the 1930s," in Getty and Manning, eds., *Stalinist Terror: New Perspectives*, 275–90.

¹¹¹ See, for instance, Bohdan Nahaylo and Viktor Swoboda, *Soviet Disunion: A History of the Nationalities Problem in the USSR* (London, 1990), chap. 6.

¹¹² Sheila Fitzpatrick, "The Impact of the Great Purges on Soviet Elites: A Case Study from Moscow and Leningrad Telephone Directories in the 1930s," 247–60, and J. Arch Getty and William Chase, "Patterns of Repression among the Soviet Elite, 1936–1939: A Biographical Approach," 225–60, both in Getty and Manning, eds., *Stalinist Terror: New Perspectives*.

¹¹³ Peter H. Solomon, "Soviet Penal Policy, 1917–1934: A Reinterpretation," *Slavic Review*, 39, no. 2 (1980): 196–217; Solomon, "Soviet Criminal Justice and the Great Terror," *Slavic Review*, 46, nos. 3–4 (1987): 391–413; Solomon, "Local Political Power and Soviet Criminal Justice, 1922–1941," *Soviet Studies*, 37, no. 3 (1985): 305–29; Eugene Huskey, "Vyshinskii, Krylenko and the Shaping of the Soviet Legal Order," *Slavic Review*, 46, nos. 3–4 (1987): 414–28.

it was in fact a penal system: a particularly harsh, cruel, and arbitrary one, to be sure, but not necessarily a one-way ticket to oblivion for the majority of inmates.

Yet it is also important to highlight three specific features. For the first, the use of capital punishment among the "measures of social defense" sets Soviet penal practices apart from those of other systems, even though the number of executions shows a sharp decrease after the dreadful dimensions in 1937–1938. Second, the detention system in the second half of the 1930s (and perhaps at other times) was directed against educated members of the elite. Third, it had a clearly political purpose and was used by the regime to silence real and imagined opponents.

Our attempt to examine the repression of the Stalin period from the point of view of social history and penology is not meant to trivialize the suffering it inflicted or to imply that it was "no better or worse" than in other authoritarian states. Although repression and terror imply issues of politics and morality, above all for those who perpetrate or justify them, we believe that scholars can also study them as a question of historical precision. The availability of new data permits us to establish more accurately the number and character of victims of the terror and to analyze the Stalinist repressive system on the basis of specific data rather than relying on the impressions and speculations of novelists and poets.¹¹⁴ We are finally in a position to begin a documented analysis of this dismal aspect of the Soviet past.

¹¹⁴ See Stephen Cohen, "Stalin's Terror as Social History," *Russian Review*, 45, no. 4 (1986): 375–84.

A Note on Sources

The GARF (TsGAOR) collection we used was that of the GULAG, the Main Camp Administration of the NKVD/MVD (the USSR Ministry of the Interior). This collection consists of nine inventories (*opisi*), the first of which, that of the Secretariat, contains the main body of accessible data on detainees. To be sure, it was not possible to scrutinize the more than 3,000 files of this *opis'*, so we restricted ourselves to those that promised to tell the most about camp populations.

Accurate overall estimates of numbers of victims are difficult to make because of the fragmentary and dispersed nature of record keeping. Generally speaking, we have runs of quantitative data of several types: on arrests, formal charges and accusations, sentences, and camp populations. But these "events" took place under the jurisdiction of a bewildering variety of institutions, each with its own statistical compilations and reports. These agencies included the several organizations of the secret police (NKVD special tribunals, known as troikas, special collegia, or the special conference [*osoboe soveshchanie*]), the procuracy, the regular police, and various types of courts and tribunals.

For example, archival data on sentences for "anti-Soviet agitation" held in different archival collections may or may not have explicitly aggregated such events by the NKVD and the civilian courts. Summary data on "political" arrests or sentences may or may not explicitly tell us what specific crimes were so defined. Aggregate data on sentences sometimes include persons who were "sentenced" (to exile or banishment from certain cities) but never formally "arrested"; when we compare sentencing and arrest data, therefore, we do not always have the information necessary to sort apples from oranges. Similarly, our task is complicated, as shown above, by the fact that many agencies sentenced people to terms in the GULAG for many different types of crimes, which were variously defined and categorized.¹¹⁵ We believe, however, that despite the lack of this information, we now have enough large chunks of data to outline the parameters and to bring the areas for which we lack data within a fairly narrow range of possibility.

Further research is needed to locate the origins of inconsistencies and possible errors, especially when differences are significant. We must note, however, that the accuracy of Soviet records on much less mobile populations does not seem to give much hope that we can ever clarify all the issues. For instance, the Department of Leading Party Cadres of the Central Committee furnished different figures for the total party membership and for its ethnic composition as of January 1, 1937, in two documents that were nevertheless compiled about the same time.¹¹⁶ Yet another number was given in published party statistics.¹¹⁷ The conditions of "perpetual movement" in the camp system created even greater

¹¹⁵ It is only after the organization of a People's Commissariat of Justice for the whole of the Soviet Union that country-wide judicial statistics become more or less trustworthy from 1937.

¹¹⁶ Compare RTsKhIDNI (TsPA), f.17, op.120, d.278, ll.8, 10; and TsKhSD (the Central Committee Archive), f.77, op.1, d.1, l.8.

¹¹⁷ *Spravochnik partiinogo rabotnika*, vyp.18 (Moscow, 1978), l.366. Since this figure corresponds to that calculated by Thomas H. Rigby, one wonders if the editors did not decide to rely more on the painstaking research of this scholar than on their own records. See *Communist Party Membership in the U.S.S.R., 1917-1967* (Princeton, N.J., 1968), 52.

difficulties than those posed by keeping track of supposedly disciplined party members who had just seen two major attempts to improve the bookkeeping practices of the party.¹¹⁸

At times, tens of thousands of inmates were listed in the category of "under way" in hard regime camp records,¹¹⁹ although the likelihood that some of them would die before leaving jail or during the long and tortuous transportation made their departure and especially their arrival uncertain. The situation is even more complicated with labor colonies, where, at any given moment, a considerable proportion of prisoners was being sent or taken to other places of detention, where a large number of convicts served short terms, and where many people had been held pending their investigation, trial, or appeal of their sentences.¹²⁰ The sources are fragmentary and scattered on colonies, but it seems that A. N. Dugin's attempt (see the Appendixes) to find figures for the beginning of each year—which was checked by V. N. Zemskov—yielded rather accurate results. Even so, we are not certain that errors have not slipped in.

Moreover, we do not know at the time of this writing if camp commandants did not inflate their reports on camp populations to receive higher budgetary allocations by including people slated for transfer to other places, prisoners who were only expected to arrive, and even the dead. Conversely, they may have reported low figures in order to secure easily attainable production targets.

We made extensive use of a series of statistics that were compiled about 1949 and that followed the evolution of a great number of parameters from 1934 up to 1948.¹²¹ We indicated some instances in which current periodic reports of the accounting department furnished slightly different figures from those of 1949 (see the notes to Tables 3, 4, and 6) and one case in which an NKVD document in 1936 gave data similar to but not entirely identical with those calculated after the war (note to Table 8). In these as well as in most other instances, the gaps are insignificant and do not call into question the orders of magnitude suggested by the postwar documents, whose figures are, as a rule, somewhat higher than the ones recorded in the 1930s. A notable exception concerns escapes, because a 1939 report mentioned almost twice as many fugitives for 1938 as the relevant table of 1949.¹²² Although we have no explanation for this discrepancy at this moment, we can speculate that the fact that a 1939 medical report showed lower mortality figures in hard regime camps in the years between 1934 and 1939 than the 1949 account may be because the latter also includes people who had been executed.¹²³

Another source we relied on consists of four tables concerning people arrested and sentenced "on cases of the secret police" from 1921 through the first half of

¹¹⁸ Getty, *Origins of the Great Purges*, 58–64, 86–90.

¹¹⁹ See, for example, GARF (TsGAOR), f.9414, op.1, d.1138, l.6.

¹²⁰ See, for instance, GARF (TsGAOR), f.9414, op.1, d.1139, ll.88–89; d.1140, l.161.

¹²¹ GARF (TsGAOR), f.9414, op.1, d.1155. For unknown reasons, the file is listed among those of 1940.

¹²² GARF (TsGAOR), f.9414, op.1, d.1140, l.53.

¹²³ GARF (TsGAOR), f.9414, op.1, d.2740, l.53. The hypothesis seems all the more tempting, since the gap widens in 1937, becomes yawning by 1938, and remains considerable in 1939. See also ll.63 and 75 with a different figure for deaths in 1939 than on l.53 and lower mortality rates in 1939–1940 than the ones given in the 1949 table.

1953.¹²⁴ A peculiarity of the document is that while enumerating sentences and arrests up to 1938, it lists fewer people arrested in 1935 and 1936 than sentenced. All the while quoting the same figure for 1935 detentions as does our source, a letter signed by the head of the NKVD also speaks of more persons against whom "proceedings [had been] instituted" than those arrested.¹²⁵ We know that some of the victims of the "cleansing" of border zones and major urban centers of "socially alien elements" had been arrested before being banished to faraway localities, although most of them seem to have been exiled without arrest by decisions of the NKVD jurisdiction.¹²⁶ We also have information in this period about defendants in affairs of "anti-Soviet" agitation who had been left free pending their trial, as well as instances of the judiciary asking the police to "resolve by administrative order" cases in which there was no legal ground for conviction,¹²⁷ a good many of which were not necessarily initiated by the NKVD.

We cannot stress enough the fact that this is only the first exploration of a huge and complex set of sources; little more than scales, ranges, and main trends of evolution can now be established. Although the above-mentioned circumstances cannot guarantee exactitude, there are good reasons for assuming that the data are reliable on the population of strict regime camps, on orders of magnitude, and on the general orientation of penal policy. There is a remarkable consistency in the way numbers, from different sources, evolve over the period under study and a notable coherence among the figures to which different types of documents refer at particular moments.¹²⁸

Moreover, figures produced by researchers using other archival collections of different agencies show close similarities in scale. Documents of the People's Commissariat of Finance discuss a custodial population whose size is not different from the one we have established.¹²⁹ In the same way, the labor force envisioned by the economic plans of the GULAG, found in the files of the Council of People's Commissars, does not imply figures in excess of our documentation.¹³⁰ Last but not least, the "NKVD contingent" of the 1937 and 1939 censuses is also consistent with the data we have for detainees and exiles.¹³¹

¹²⁴ V. P. Popov published part of these tables ("Gosudarstvennyi terror v sovetskoi Rossii, 1923–1953 gg.," *Otechestvennye arkhivy*, no. 2 [1992]: 28). Besides combining several columns that masked many significant details, this publication lists eight executions for 1949, although none are given in the source for that year. Capital punishment was abolished in May 1947 and reintroduced in early 1950 (*Vedomosti Verkhovnogo Soveta SSSR*, no. 17 [1947]: 1; no. 3 [1950]: 1).

¹²⁵ GARF (TsGAOR), f.8131sch, op.27, d.70, l.138. Nevertheless, this document gives slightly higher figures for the number of convictions by different police bodies than the 1953 table. An explanation for this circumstance may be that the letter was written in early 1936, when the outcome of certain appeals was not clear.

¹²⁶ GARF (TsGAOR), f.8131sch, op.27, ll.135, 139; d.58, l.138; d.59, l.187.

¹²⁷ GARF (TsGAOR), f.9474, op.1, d.97, l.7; "Smolensk Archive," WKP 237, 55.

¹²⁸ See, for example, GARF (TsGAOR), f.8131sch, op.27, d.70, ll.104, 141; f.9414, op.1, d.20, ll.135, 149.

¹²⁹ V. V. Tsaplin, "Arkhivnye materialy o chisle zakliuchennykh v kontse 30-kh godov," *Voprosy istorii*, nos. 4–5 (1991): 157–60.

¹³⁰ See Oleg V. Khlevniuk, "Prinuditel'nyi trud v ekonomike SSSR, 1929–1941 gody," *Svobodnaia mysl'*, no. 13 (1992): 73–84.

¹³¹ See E. M. Andreev, L. E. Darskii, and T. L. Khar'kova, *Istoriia Naseleniia SSSR 1920–1959 gg.* (vypusk 3–5, chast' I, of *Ekspress-informatsiia, seriia: Istoriia statistiki*) (Moscow, 1990), 31, 37; V. N. Zemskov, "Ob uchete spetskontingenta NKVD vo vsesoiuznykh perepisiakh naseleniia 1937 i 1939 gg.," *Sotsiologicheskie issledovaniia*, no. 2 (1991): 74–75.

Appendix (a). USSR Custodial Populations, 1934-1943

	1934	1935	1936	1937	1938	1939	1940	1941	1942	1943
GULAG CAMPS										
Jan. 1 population to the GULAG from:	510,307	725,483	839,406	820,881	996,367	1,317,195	1,344,408	1,500,524	1,415,596	983,974
NKVD camps	100,389	67,265	157,355	211,486	202,721	348,417	498,399	488,964	246,273	114,152
Other places of detention	445,187	409,663	431,442	636,749	803,007	383,994	644,927	840,712	544,583	355,728
Recaptures	46,752	45,988	35,891	35,460	22,679	9,838	8,839	6,528	4,984	3,074
Other	1,374	1,412	1,381	1,116	7,758	7,398	6,237	7,459	10,207	4,221
from the GULAG to:										
NKVD camps	103,002	72,190	170,484	214,607	240,466	347,444	563,338	540,205	252,174	140,756
Other places of detention	17,169	28,976	23,826	43,916	55,790	74,882	57,213	135,537	186,577	140,093
Freed	147,272	211,035	369,544	364,437	279,966	223,622	316,825	624,276	509,538	336,153
Died	26,295	28,328	20,595	25,376	90,546	50,502	46,665	100,997	248,877	166,967
Escaped	83,490	67,493	58,313	58,264	32,033	12,333	11,813	10,592	11,822	6,242
Other	1,298	2,383	1,832	2,725	16,536	13,651	6,432	16,984	12,917	7,344
Dec. 31 population:	725,483	839,406	820,881	996,367	1,317,195	1,344,408	1,500,524	1,415,596	983,974	663,594
Annual change	215,176	113,923	-18,525	175,486	320,828	27,213	156,116	-84,928	-431,622	-320,380
death rate/1000	52	39	25	31	91	38	35	67	176	170
"Counterrevolutionaries":										
Jan. 1 population	135,190	118,256	105,849	104,826	185,324	454,432	444,999	420,293	407,988	345,397
Annual change	-16,934	-12,407	-1,023	80,498	269,108	-433	-24,706	-12,305	-62,591	-76,536
Percent of GULAG population	26.5	16.3	12.6	12.8	18.6	34.5	33.1	28.7	29.6	35.6
LABOR COLONIES										
Jan. 1 population	-	240,259	457,088	375,488	885,203	355,243	315,584	429,205	360,447	500,208
Dec. 31 population	-	457,088	375,488	885,203	355,243	315,584	429,205	360,447	500,208	516,225
Annual change	-	216,829	-81,600	509,715	-529,960	-39,659	113,621	-68,758	139,761	16,017
PRISONS										
Jan. 15 population	-	-	-	-	-	350,538	190,266	487,739	277,992	235,313

Appendix (b). USSR Custodial Populations, 1943-1953

	1944	1945	1946	1947	1948	1949	1950	1951	1952	1953
GULAG CAMPS										
<i>Jan. 1 population to the GULAG from:</i>										
NKVD camps	663,594	715,506	600,897	808,839	1,108,057	1,216,361	1,416,300	1,533,767	1,711,202	1,727,970
Other places of detention	48,428	59,707	172,844	121,633	213,102	564,800	561,660	657,557	603,093	393,504
Recaptures	326,928	361,121	461,562	624,345	482,498	88,235	71,339	55,291	14,849	16,853
Other	1,839	953	1,203	1,599	2,494	1,733	1,723	1,341	905	415
<i>from the GULAG to:</i>	2,394	2,136	579	1,043	870	1,054	1,329	833	5	—
NKVD camps	64,110	96,438	182,647	153,899	203,938	239,762	258,269	250,836	221,619	278,240
Other places of detention	39,303	70,187	99,332	58,782	100,901	16,344	16,882	21,845	15,836	8,934
Freed	152,131	336,750	115,700	194,886	261,148	178,449	216,210	254,269	329,446	937,352
Died	60,948	43,848	18,154	35,668	27,605	15,739	14,703	15,587	10,604	5,825
Escaped	3,586	2,196	2,642	3,779	4,261	2,583	2,577	2,318	1,253	785
Other	7,590	6,105	9,771	2,388	2,162	3,006	333	295	578	1,949
<i>Dec. 31 population:</i>	715,506	600,897	808,839	1,108,057	1,216,361	1,416,300	1,533,767	1,711,202	1,727,970	897,051
Annual change	51,912	-114,609	207,942	299,218	108,304	199,939	117,467	117,435	16,768	-830,919
<i>death rate/1000</i>	92	61	30	44	25	13	10	10	6	3
<i>"Counterrevolutionaries":</i>										
Jan. 1 population	268,861	283,351	333,833	427,653	416,156	420,696	578,912	475,976	480,766	465,256
Annual change	14,490	50,482	93,820	-11,497	4,540	153,216	—	4,790	-15,510	—
Percent of GULAG pop.	40.7	41.2	59.2	54.3	38.0	34.9	22.7	31.0	28.1	26.9
LABOR COLONIES										
Jan. 1 population	516,225	745,171	956,224	912,794	1,091,478	1,140,324	1,145,051	994,379	793,312	740,554
Dec. 31 population	745,171	956,224	912,794	1,091,478	1,140,324	1,145,051	994,379	793,312	740,554	463,252
Annual change	228,946	211,053	-43,430	178,684	48,846	4,727	-150,672	-201,067	-52,758	-277,302
PRISONS										
Jan. 15 population	155,213	279,969	261,500	306,163	275,850	—	—	—	—	—

NOTE: The 1938 data for the population of colonies also includes prison inmates, who numbered 548,417 on February 10, 1938, and the 1946 population, which contains 444,500 persons sentenced to "corrective work" without detention; GARF (TsGAOR), fond 9414, opis' 1, delo 330, listy 55; d. 1139, l. 88; d. 1259, l. 18). The 1950 figure for "politicals" includes detainees in labor colonies. Camp and colony data are unavailable for December 31, 1953, and are here replaced by the numbers for April 1, 1954, when 448,344 "counterrevolutionaries" were held at these places of detention.

SOURCES: GARF (TsGAOR), fond 9414, opis' 1, delo 1155, listy 2-3 (camps and "counterrevolutionaries," 1934-47); d. 1190, l. 36; d. 1319, ll. 2-15, d. 1356, ll. 2-3 (camps and "counterrevolutionaries," 1948-53); f. 9413, op. 1, d. 11, ll. 1-10 (prisons); A. N. Dugin and A. Ia. Malygin, "Solzhnitsyn, Rybakov: Tekhnologiya Izhi," *Voenna-istoricheskii zhurnal*, no. 7, 1991, 68-70 (for colonies: calculations verified by V. N. Zemskov on the basis of GARF (TsGAOR), f. 9414, op. 1, d. 330, l. 55). See also A Note on Sources.

Distant Thunder: The Creation of a World Market in Rice and the Transformations It Wrought

PETER A. COCLANIS

Come listen, all you darkies, come listen to my song,
It am about ole Massa, who use me bery wrong:
In de cole, frosty mornin', it an't so bery nice,
Wid de water to de middle to hoe among de rice

from a slave song,
South Carolina Low Country

Planting rice is never fun;
Bent from morn till set of sun;
Cannot stand and cannot sit;
Cannot rest for a little bit.
Oh, my back is like to break,
Oh, my bones with dampness ache,
And my legs are numb and set
From the soaking in the wet.

from an old Filipino song¹

RICE (*ORYZA SATIVA*) HAS SHAPED THE LIVES OF RELATIVELY FEW WESTERNERS over time. It has dominated the lives of fewer still. While the cereal has been known in the West since antiquity, its production and consumption for the most part have been of only minor importance, occurring at the margin of Western foodways. That we speak of breadwinners rather than ricewinners and pray for our daily bread rather than our daily rice tells us something about the hold of bread—primarily wheat bread—on the Western world. In the East, where the rice plant originated, things are far different; with all due respect to James Henry Hammond, in that part of the world, rice is indeed king. That the Indian word for rice, *dhanya*, means “sustainer of the human race,” that the name of the Buddha’s father, Suddhodana, the sixth-century–B.C. king of Nepal, literally means “pure rice,” and that the idiomatic expression “Have you eaten your rice today?” was a polite way of saying hello in traditional Chinese society only begins to convey the place of rice in the East.²

¹ The slave song can be found in Edmund Kirke [James Roberts Gilmore], *Among the Pines; or, South in Secession-Time* (New York, 1862), 22. The Filipino song is from Edith Fowke and Joe Glazer, eds., *Songs of Work and Protest* (New York, 1973), 102–03.

² See D. H. Grist, *Rice*, 5th edn. (London, 1975), 3–10; Lucien M. Hanks, *Rice and Man: Agricultural Ecology in Southeast Asia* (Arlington Heights, Ill., 1972), 16–22; E. J. Kahn, Jr., *The Staffs of Life* (Boston, 1985), 210, 212, 219.

Why bother to study rice from the vantage point of the West? For two interrelated reasons. First, because the evolution of the market for rice in the West offers insight into the expansion and elaboration of capitalism, the most important economic development in the last five hundred years. To be sure, the rice market in the West was tiny in comparison to the market for small grains. Even in the early twentieth century, after centuries of market development, rice accounted for but a small fraction of the total trade in cereals, under 10 percent. Moreover, since small grains and rice are basically similar in caloric content—rice contains between about 354 and 362 kilocalories per 100 grams, and most small grains contain between about 320 and 365—rice, measured in terms of trade volume or caloric importance, was at best a minor component of the Western grain trade.³

But importance is not reducible to and cannot be deduced from numbers alone. Indeed, the case of rice is instructive and illuminating not so much because of its scale but because of its increasingly global scope. This brings me to the second reason: because a study of the rice market in the West requires that we look east, such a study provides an argument for the utility of a truly international approach to history. Regional or national approaches to the subject, self-contained and self-absorbed as they often are, will not do in this case.

Before we can begin to understand the market for rice in the West, we must know something of the uses to which the cereal was put. It is difficult to generalize about demand for any commodity or product but particularly so in the case of demand for rice in the West. Because of rice's relative insignificance in comparison to small grains and maize and its many inconspicuous or intermediate usages, historical patterns of rice consumption in the West remain somewhat murky even today. We know, for example, that rice served certain ceremonial functions, while a variety of sources point to its pharmaceutical uses. Still other sources suggest that at certain times and in certain places, rice was considered a luxury item, the demand for which varied directly rather than indirectly with price.⁴ Neither ceremony nor pharmacy nor luxury, however, will go very far toward explaining

³ Figures on the world grain trade are inexact, but available data do convey the order of magnitude of the rice trade in the West. Between 1902 and 1911, for example, rice shipments from Southeast Asia to the West averaged about 1.454 million metric tons annually. Available data suggest that this figure should be raised by another 4 percent or so to account for rice exports from Bengal and other Eastern supply sources during this period. The West's six largest wheat exporters shipped about 14.8 million metric tons of wheat annually between 1909 and 1913. There were other wheat exporters and, of course, other small grains besides wheat. See Randolph Barker, *et al.*, *The Rice Economy of Asia*, 2 vols. (Washington, D.C., 1985), 1: 187; Frank M. Surface, *The Grain Trade during the World War* (New York, 1928), 22; Imperial Institute, Indian Trade Enquiry, *Reports on Rice* (London, 1920), 8, 35–62. On the caloric values for rice and other cereals, see Arnold E. Bender, *Dictionary of Nutrition and Food Technology*, 6th edn. (London, 1990), 245, 302–03; Daniel N. Lapedes, ed., *McGraw-Hill Encyclopedia of Food, Agriculture, and Nutrition* (New York, 1977), 697, 699, 704, 705.

⁴ See William Shakespeare, *The Winter's Tale* (c. 1610–11), act 4, scene 3, lines 39–44; Thomas Elyot, *The Castel of Helth, Corrected and in some places augmented by the fyrste authour therof* (London, 1541), fyrste boke, 15; Francis Bacon, *Sylva Sylvarum; or, A Naturall Historie, in Ten Centuries* (London, 1627), century 1.49, 16; John Arbuthnot, *Practical Rules of Diet in the Various Constitutions and Diseases of Human Bodies* (London, 1736), 256, 301–02; Alexis Soyer, *The Pantropheon; or, History of Food . . .* (London, 1853), 43; Jost Amman and Hans Sachs, *Eygentliche Beschreibung aller Stände auff Erden* (Frankfurt am Main, 1568), entry "Der Koch"; V. D. Wickizer and M. K. Bennett, *The Rice Economy of Monsoon Asia* (Stanford, Calif., 1941), 15.

the West's demand for rice. Its true importance derives from its transformation in the early modern period into an everyday commodity with numerous alimentary and industrial uses.

Rice and its by-products found employment in the starch and paper industries, for example, and were used extensively for animal feed. In the eighteenth century, liqueurs such as brandies and arrack often included rice among their ingredients, and in the second half of the nineteenth century rice gained widespread acceptance in the brewing industry. To the middle class, whose incomes allowed for dietary concerns that transcended mere subsistence, rice became a ready nutritional complement or supplement. In all likelihood, however, its greatest food use in the eighteenth and nineteenth centuries was as a versatile and relatively cheap dietary staple, especially useful for feeding commoners and *lumpen* groups—orphans, soldiers, sailors, inmates, the poor—in the absence of, or instead of, other cereals. Authorities ranging from Nicolas Baudeau in the eighteenth century to the writers of the *Encyclopaedia Britannica* in the early nineteenth century to Fernand Braudel in the late twentieth century agree on these basic points.⁵

This said, one must be careful about focusing too much attention on any single source of demand. Some scholars, cognizant of the fact that rice—until recently at least—has been considered highly income-inelastic, have focused, it is true, solely on this last, *lumpen* source of demand.⁶ However, rice demand was general rather than class-specific during periods of crop failure and shortfalls in the West, and it played an intermediary role in a number of foods and industrial products of greater income elasticity. Further complexities arise from the fact that much of the “lumpen” demand for rice actually emanated from other social groups. From this perspective, a new or at least enhanced social preference for provisioning the putative dregs of society—sometimes with rice—can itself be viewed as a kind of income-elastic “commodity.”⁷

Clearly, then, caution is still called for when speaking of rice demand. That rice prices throughout the West generally varied directly with the prices of wheat and

⁵ On the uses of rice in Europe and the Americas in the eighteenth and nineteenth centuries, see James Glen, *A Description of South Carolina . . .* (London, 1761), 91; Nicolas Baudeau, . . . *Commerce . . .*, 3 vols. (Paris, 1783–84), 3: 588–89; Court of Directors to Governor General and Council at Fort William, April 5, 1793, in *Indian Records Series: Fort William-India House Correspondence . . .* (Public Series), 21 vols. (Delhi, 1949–85), vol. 12, 1793–95, 55; *Encyclopaedia Britannica*, 3d edn., 20 vols. (Edinburgh, 1797–1801), Supplement 2 (vol. 20), 462; [Great Britain] *House of Commons Sessional Papers of the Eighteenth Century, Reports & Papers*, vol. 131: *George III, Food Supply, Fisheries 1799–1800 and 1800*, Sheila Lambert, ed. (Wilmington, Del., 1975), 65–68, 353–58, 367–73, 445–65, 519–22; (London) *The Times*, November 9, 1811; Fernand Braudel, *Civilization and Capitalism 15th–18th Century*, 3 vols., Siân Reynolds, trans. (New York, 1981–84), 1: 109–14, 145–58.

⁶ On the income inelasticity of rice, see, for example, Theodore W. Schultz, *The Economic Organization of Agriculture* (New York, 1953), 71–73.

⁷ See Peter A. Coclanis, *The Shadow of a Dream: Economic Life and Death in the South Carolina Low Country, 1670–1920* (New York, 1989), 53–54, 239. That rice commonly was consumed in hospitals for the poor and aged and in orphanages in early modern Europe is apparent from data available in standard works on European prices. See, for example, William Beveridge, *Prices and Wages in England from the Twelfth to the Nineteenth Century* (London, 1939), 255, 292, 540; N. W. Posthumus, *Inquiry into the History of Prices in Holland*, 2 vols. (Leiden, 1946–64), 2: 459–66, 773, 781, 785, 788, 790, 792, 794, 795. On rice consumption in British prisons in the nineteenth century, see Valerie Johnston, “The Diets of the Local Prisons 1835 to 1878,” in Derek J. Oddy and Derek S. Miller, eds., *Diet and Health in Modern Britain* (London, 1985), 207–30.

other small grains in the eighteenth and nineteenth centuries can readily be established; precisely why this was so remains more conjectural.⁸

IF CONSUMER PREFERENCES AND THE AVAILABILITY OF NUMEROUS SUBSTITUTES rendered rice less than indispensable in the West, its price and versatility generally rendered it vendible throughout the Western world. In the eighteenth and nineteenth centuries—the period of primary concern here—rice was traded over a vast area from Peru and Argentina to the shores of the Black Sea. During this entire period, demand emanating from northern Europe proved central to the workings of the Western rice trade. To be sure, substantial demand existed in other areas—North America, southern Europe, the West Indies, and Brazil especially—but it is within the context of northern Europe's massive grain market that the rice trade can best be construed.

Unfortunately, it is impossible to chart precisely the labyrinthine course of commodity flows within this market in the eighteenth and nineteenth centuries. Extant data, scattered widely in customs records, toll registers, and commercial manuals and journals, indicate, however, that Germany was the center of consumption in northern Europe, with large quantities of rice entering the various German states from such ports as Amsterdam, Rotterdam, Hamburg, Bremen, and Danzig. Significant amounts of rice were consumed elsewhere in the North: with regularity in France, Belgium, and the Netherlands, for example, and, as the Sound tolls reveal, at least intermittently in Scandinavia and the eastern Baltic as well. Internal consumption in Great Britain, which traditionally had been quite limited, increased in the nineteenth century for a variety of reasons. Some shipments of rice, having worked their way down the Danube, made it to the ports of Ibraila and Galatz on the Black Sea, but shipments to that region did not compare to shipments to the west and north. We still need to ask whence and how the West's demand for rice was met and how supply changed over time.⁹

⁸ See Arthur H. Cole, *Wholesale Commodity Prices in the United States, 1700–1861*, 2 vols. (Cambridge, Mass., 1938), 2: 15–69; Posthumus, *Inquiry into the History of Prices in Holland*, 1: 2–8; Vitorino Magalhães Godinho, “Preços e conjuntura do século XV ao XIX,” in *Dicionário de história de Portugal*, 4 vols. (Lisbon, [1971?]), 4: 488–516, esp. 506, 508–09; Godinho, *Prix et monnaies au Portugal 1750–1850* (Paris, 1955), 72–78, figures following 371; Henri Hauser, *Recherches et documents sur l'histoire des prix en France de 1500 à 1800* (1936; Geneva, 1985), 97–135; Beveridge, *Prices and Wages in England*, 734–35; Braudel, *Civilization and Capitalism*, 1: 109–14; Luigi Faccini, *L'economia risicola lombarda dagli inizi del XVIII secolo all'Unità* (Milan, 1976), 23–26; A. J. H. Latham and Larry Neal, “The International Market in Rice and Wheat, 1868–1914,” *Economic History Review*, 2d ser., 36 (May 1983): 160–80; U.S. Department of Agriculture, Bureau of Agricultural Economics, *Gross Farm Income and Indices of Farm Production and Prices in the United States, 1869–1937*, by Frederick Strauss and Louis H. Bean, Technical Bulletin No. 703 (Washington, D.C., 1940), 36–40, 69–71.

⁹ See Glen, *Description of South Carolina*, 90–93; (Charleston) *South Carolina Gazette*, January 10, 1761; Marie Scholz-Babisch, *Quellen zur Geschichte des Klevischen Rheinzollwesens vom 11. bis 18. Jahrhundert*, 2 vols. (Wiesbaden, 1971), 2: 870, 912, 997, 999; Nina E. Bang and Knud Korst, *Tabeller over skibsfart og varetransport gennem Øresund, 1661–1783, og gennem Storebaelt 1701–1748*, 4 vols. (Copenhagen, 1930), 2, part 2, *passim*; Ralph Izard to Thomas Jefferson, June 10, 1785, in Julian P. Boyd, et al., eds., *The Papers of Thomas Jefferson*, 25 vols. (Princeton, N.J., 1950–), 8: 195–204; H. P. H. Nusteling, *De Rijnvaart in het tijdperk van stoom en steenkool 1831–1914* (Amsterdam, 1974), 123–79 (esp. 132, 153–55), 464; *Hunt's Merchants' Magazine*, 21 (July 1849): 104; *De Bow's Review*, 25

It is clear that rice was traded in the West well before its commodification, indeed, well before it was even produced in that part of the world. Mentioned in the Talmud but, interestingly enough, not in the Bible, rice was imported from Syria, Persia, and India by both the Greeks and the Romans. While some believe that rice was produced in Sicily in ancient times, most authorities believe that European cultivation began in earnest only with the Moors' incursion into Spain in the eighth century and the concomitant Turkish thrust into southeastern Europe and the Balkans.¹⁰ Rice cultivation spread to other parts of Mediterranean Europe from these points of entry, and Europeans continued, intermittently, to import rice from the East—the Saracens were particularly important in the trade—throughout the medieval period. It was not until cultivation began in northern Italy in the fifteenth century, however, that the European rice industry attained more than local significance in economic terms.¹¹ European production has centered in the rich alluvial valleys of the upper Po, that is, in Piedmont and Lombardy, ever since. In good harvest years during the sixteenth and seventeenth centuries, northern Italian rice was shipped both to other parts of the peninsula and to other parts of Europe, much of it being distributed by Rhineland traders as well as by the German Hanse.¹² Such trade notwithstanding, rice was of limited importance in the West as the early modern period began. Europe's outward thrust during that period transformed the economic geography of rice supply in a variety of ways. As implied earlier, the economic dynamism that at once underpinned and reinforced this thrust eventually helped transform demand for rice in the West.

The reasons for the economic dynamism in parts of Europe during the early modern period lie well beyond the scope of this article. Suffice it to say that, as a result of a multiplicity of factors ranging from increased agricultural productivity to a secular decline in transactions costs, these areas began to achieve relatively sustained and fairly robust rates of growth in output and income per capita. Over time, the positions and structures of both supply and demand in these areas shifted in ways that encouraged and facilitated expanded economic contacts with

(September 1858): 350–51; Peter Buss to Robert Pringle, February 14, 1784, Pringle Family Papers, South Carolina Historical Society, Charleston, S.C.; *Hunt's Merchants' Magazine*, 11 (August 1844): 179–80; 16 (February 1847): 138–51, esp. 148–49; 23 (August 1850): 177–88; 31 (November 1854): 558–65; 13 (August 1845): 193–96; 18 (April 1848): 413–15; 43 (July 1860): 91–94; Lewis C. Gray, *History of Agriculture in the Southern United States to 1860*, 2 vols. (Gloucester, Mass., 1958), 1: 286; *Hunt's Merchants' Magazine*, 27 (September 1852): 289–309; 28 (January 1853): 81–86.

Most of the rice imported into Great Britain during the eighteenth century was re-exported to the Continent. On the reasons for the rise of domestic rice demand in Great Britain in the nineteenth century, see below.

¹⁰ Grist, *Rice*, 5–6; E. H. Warmington, *The Commerce between the Roman Empire and India*, 2d edn. (New York, 1974), 40, 218–19; Norman J. G. Pounds, *An Historical Geography of Europe 450 B.C.–A.D. 1330* (Cambridge, 1973), 69. Also see David MacPherson, *Annals of Commerce, Manufactures, Fisheries and Navigation*, 4 vols. (London, 1805), 1: 162.

¹¹ Grist, *Rice*, 6; "Riso," in *Enciclopedia italiana di scienze, lettere ed arti*, 36 vols. plus appendixes and index (Rome, 1929–52), 29 (1936): 424–31; Faccini, *L'economia risicola lombarda*; Peiraldo Bullio, "Problemi e geografia della risicoltura in Piemonte nei secoli XVII e XVIII," *Annali della Fondazione Luigi Einaudi*, 3 (1969): 37–93; Ira A. Glazier, *Il commercio estero del regno Lombardo-Veneto dal 1815 al 1865* (Rome, 1966), 30–45 and *passim*.

¹² See the works cited in footnote 11. Also see Philippe Dollinger, *The German Hansa*, D. S. Ault and S. H. Steinberg, trans. and eds. (Stanford, Calif., 1970), 212–13, 223 especially.

other parts of the world. The commercial aspects of what we today refer to as the Age of Discovery, particularly in its less feverish phase during the seventeenth and eighteenth centuries, can for our purposes be viewed as a rational response to perceived economic opportunities.

This led in the East to the establishment of European trading factories and the rerouting to some degree of intra-Asian trade; in the West, to direct European involvement in, or at least supervision of, production itself.¹³ It led, too, to the beginnings of factor and product markets of global scope. Such markets were at first highly imperfect. Long distance, intercontinental trade predated the era of the English East India Company and the V.O.C. (Vereenigde Oost-indische Compagnie) by a millennium or two. And, prior to the seventeenth and eighteenth centuries, such trade, generally speaking, was quantitatively insignificant, often entailing the intermittent or periodic exchange of preciousities of relatively limited market purview.

In the last half of the early modern era, the value and quality of such trade changed dramatically, as Europeans generally and European merchant capital specifically responded to economic change at home. Economic growth in parts of Europe, most notably in the northwest, created conditions necessary and sufficient to allow for both the global reallocation of European labor and capital and the emergence of a loosely integrated international trading system organized and dominated by Europeans and their legatees. While this system ultimately affected world trade in innumerable ways, it is enough to note here that European merchant capital, supported when necessary by state power, succeeded during this period in regularizing and routinizing such trade and in shifting its emphasis from high-value preciousities to bulk commodities, of which rice was one.

AS EUROPEANS CAST ABOUT THE GLOBE for profitable economic opportunities, an amazing array of inorganic and organic substances were put to the test. Various minerals ultimately achieved commercial importance, as did such cultigens as sugar, tobacco, indigo, cotton, and rice. The last of these, rice—a plant that can be grown virtually anywhere—was tried in such unlikely and seemingly unrelated places as the central Andes, the western part of Ruthenia, and the tidewater of Virginia. But probably by the middle of the eighteenth century, the British colony of South Carolina had become the West's leading rice exporter. This colony, along with neighboring Georgia, which soon became a major exporter as well, possessed a number of advantages that help to explain why rice proved successful there at that time.¹⁴

¹³ See Immanuel Wallerstein, *The Modern World-System*, 3 vols. (New York, 1974–); M. A. P. Meilink-Roelofs, *Asian Trade and European Influence in the Indonesian Archipelago between 1500 and about 1630* (The Hague, 1962); K. M. Panikkar, *Asia and Western Dominance* (London, 1959), 13–174; Holden Furber, *Rival Empires of Trade in the Orient, 1600–1800* (Minneapolis, Minn., 1976); Anthony Reid, *Southeast Asia in the Age of Commerce 1450–1680*, Vol. 1: *The Lands below the Winds* (New Haven, Conn., 1988), 1–119.

¹⁴ On the global reallocation of European labor and capital, see Wallerstein, *Modern World-System*. On the dissemination of rice by Europeans, see, for example, Wickizer and Bennett, *Rice Economy of Monsoon Asia*, 13–16; Grist, *Rice*, 5–8; Kahn, *Staffs of Life*, 220–24. Note that rice exports from

First of all, the physical environment of the lower coastal plain of both South Carolina and Georgia was highly conducive to rice production. Given the region's humid subtropical climate, abundant precipitation, seemingly inexhaustible surface water resources, and loamy Ultisols (red-yellow podzols), it would have been surprising, other things being equal, if rice had not emerged as an important export commodity. This seems particularly understandable in light of the region's human and nonhuman capital resources and its market position by the mid-eighteenth century. An ample, cost-efficient, disciplined, and technologically skilled African and creole labor force was available by that time, at least some members of which had cultivated rice (whether *O. sativa* or a related species, *O. glaberrima*) in West Africa. A powerful class of planters and merchants, eager for profits, had shown itself capable of organizing production with method and rigor, and European and American merchant capital had done likewise, greatly facilitating long-distance trade. Finally, and perhaps most important, transport costs, market information, and the degree of integration of what was still a rudimentary world economy favored this region over several other rice-producing areas.¹⁵

Things would change, however. During the first half of the nineteenth century, the South Carolina–Georgia rice region, despite producing more rice than ever before, saw itself surpassed as a supply source for the principal Western markets. The region's relative decline in international competitiveness was due in part to internal supply constraints. The amount of land suitable for rice cultivation—cultivation increasingly dependent on tidal irrigation technology—was limited by geography, and, by the second quarter of the nineteenth century, some of the best land in the tidal zone was losing fertility. Furthermore, the simultaneous expansion westward of the southern cotton industry not only forced coastal rice planters to compete for outside capital with a more dynamic industry but actually siphoned a good part of indigenous capital and entrepreneurship from the rice area itself.

It would nonetheless be a mistake to focus solely on internal constraints in explaining the relative decline of the South Carolina–Georgia rice region, for the economic geography of international rice supply was being drastically transformed at the same time. Obviously, markets are never completely static, and the South Carolina–Georgia region faced competitive challenges even during its heyday in the eighteenth century. The region had lost the Portuguese market to Brazil in the 1790s, and Italian rice claimed segments of the European market. The stagnation and ultimate demise of the South Atlantic rice industry was not chiefly a result of competition from these areas, however, but from distant lands, namely, Bengal, Java, and, later, Lower Burma, Siam, and Indochina. As global market integration proceeded, these Southeast Asian rice-producing areas were increasingly incorporated into the West's economic orbit, setting into motion forces from which the South Carolina–Georgia rice economy failed to recover.¹⁶

northern Italy in the eighteenth century varied considerably from year to year but were sometimes impressive. Quantitative data from the period are extremely sketchy, however. See Faccini, *L'economia risicola lombarda*, 23–26.

¹⁵ Coclanis, *Shadow of a Dream*, 27–38, 48–110.

¹⁶ See Coclanis, *Shadow of a Dream*, 111–58. On the Brazilian rice industry, see Paul I. Mandell,

Asian rice had long been arriving in Europe, both via the Levant and transoceanic routes, but shipments increased dramatically once British economic and political control of Bengal intensified in the late eighteenth century. The story of Britain's gradual but seemingly inexorable incorporation of Bengal into the world market after Robert Clive's famous victory at Plassey in 1757 is already well known. While Clive's subsequent procurement in 1765 of the *diwani*—the right of revenue collection in Bengal, Bihar, and Orissa—may have been more important than the actual events of 1757, a key theme of the entire post-Plassey period is the heightened flow of both capital and raw materials from eastern India to Great Britain. Included in the category of raw materials were a variety of spices, saltpeter, and agricultural commodities such as indigo, sugar, cotton, flax, hemp, and, of course, rice. For our purposes, it is important to note that the process of market incorporation proceeded relatively rapidly in the case of rice: even before the 1820s, rice produced by Bengali peasants was cutting significantly into South Carolina's and Georgia's markets in northern Europe.¹⁷ The degree to which such rice penetrated the British market in the first half of the nineteenth century is illustrated in Tables 1 and 2. It is also important to note—as David MacPherson did in 1795—that rice was the first “necessary” sent to the West from India, all previous trade consisting of articles and products “rather of ornament and luxury than of use.”¹⁸

To what, primarily, was Bengal's rapid incorporation into Western rice markets due? In part, certainly, to aggressive entrepreneurship by British (English and Scottish) merchant capital and to aggressive action on the part of the British state. The British meant business in Bengal. No one saw this more clearly than Karl Marx, whose piercing observation—“They speak of God. They mean Cotton”—cuts right to the core.

But there were other reasons as well. Without the collaboration of Indian merchant capital, for example, the British never would have succeeded in redirecting a portion of Bengal's agricultural production to Europe. Without significant changes in Western supply and demand, they might not even have tried. An unusual conjuncture of short-run and long-term factors was needed to create the context for such success.

Supply disruptions arising, first, from the American Revolution and, shortly thereafter, from the Napoleonic wars, interrupted, impeded, and, at times,

“The Rise of the Modern Brazilian Rice Industry: Demand Expansion in a Dynamic Economy,” *Stanford Food Research Institute Studies*, 10 (1971): 161–219. Note that data on rice exports from northern Italy for the period between 1800 and 1861 are also very sketchy. See Tom P. Barbiero, “A Reassessment of Agricultural Production in Italy, 1861–1914: The Case of Lombardy,” *Journal of European Economic History*, 17 (Spring 1988): 103–16; Gianni Toniolo, *An Economic History of Liberal Italy 1850–1918*, Maria Rees, trans. (London, 1990), 39–40. Also note, however, that Lombardy and Venetia apparently were net importers of rice at times during the 1815–1865 period. See Glazier, *Il commercio estero del regno Lombardo-Veneto*, 30–45.

¹⁷ On the British penetration of Bengal, see Narendra Krishna Sinha, *The Economic History of Bengal: From Plassey to the Permanent Settlement*, 2 vols. (Calcutta, 1961–62); Amales Tripathi, *Trade and Finance in the Bengal Presidency 1793–1833*, rev. edn. (Calcutta, 1979); C. A. Bayly, *Indian Society and the Making of the British Empire* (Cambridge, 1988). On rice exports from Bengal to Europe in the early nineteenth century, see John Phipps, *A Guide to the Commerce of Bengal* (Calcutta, 1823), 211, 223–24; [Great Britain] *House of Commons Parliamentary Papers 1828*, vol. 18: 379–86.

¹⁸ MacPherson, *Annals of Commerce*, 4: 362.

Table 1. Rice Imports into England, Wales, and Great Britain, 1696–1808

A. Imports into England and Wales, 1696–1780									
Period	Total (cwts)								
1696–1700	30,653								
1701–1710	60,172								
1711–1720	259,022								
1721–1730	850,892								
1731–1740	1,617,931								
1741–1750	1,495,092								
1751–1760	1,758,950								
1761–1770	3,037,039								
1771–1780	2,434,400								
Imports by Region (% of total)									
	1696– 1700	1701– 1710	1711– 1720	1721– 1730	1731– 1740	1741– 1750	1751– 1760	1761– 1770	1771– 1780
The “Thirteen Colonies”	13.95	89.19	94.79	99.06	99.79	99.35	99.30	99.13	98.57
Other North America	0.01	0.00	0.01	0.00	0.00	0.00	0.00	0.01	0.47
West Indies and Central America	0.16	1.14	4.14	0.75	0.19	0.20	0.66	0.57	0.64
South America	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Southern Europe and Atlantic Islands	83.23	8.88	0.72	0.12	0.00	0.37	0.00	0.17	0.10
Northern Europe	1.32	0.08	0.11	0.00	0.00	0.02	0.03	0.01	0.18
Africa	0.00	0.00	0.06	0.01	0.00	0.00	0.00	0.10	0.02
Asia	<u>1.32</u>	<u>0.71</u>	<u>0.17</u>	<u>0.05</u>	<u>0.01</u>	<u>0.07</u>	<u>0.00</u>	<u>0.00</u>	<u>0.02</u>
	99.99%	100.00%	100.00%	99.99%	99.99%	100.01%	99.99%	99.99%	100.00%
B. Imports into Great Britain, 1772–1808									
Period	Total (cwts)								
1772–1780	2,004,769								
1781–1790	1,344,608								
1791–1800	1,983,633								
1801–1808	1,281,533								
Imports by Region (% of total)									
	1772–1780	1781–1790	1791–1800	1801–1808					
The “Thirteen Colonies”	98.31	98.40	89.31	67.02					
Other North America	0.51	0.01	0.32	0.07					
West Indies and Central America	0.80	0.72	0.05	0.12					
South America	0.00	0.00	0.00	1.47					
Southern Europe and Atlantic Islands	0.12	0.40	0.26	0.12					
Northern Europe	0.22	0.04	1.04	0.24					
Africa	0.02	0.04	0.26	0.10					
Asia	<u>0.02</u>	<u>0.38</u>	<u>8.76</u>	<u>30.85</u>					
	100.00%	99.99%	100.00%	99.99%					

SOURCES: Customs 3/1-82, Customs Office Records, Public Record Office, London, England; Customs 17/1-30, Customs Office Records, Public Record Office. Note that data for 1705 and 1712 no longer survive. Given the purposes of this exercise, I have made no adjustments or estimates for these years. Also note that I have excluded rice taken on "prize vessels" during periods of war. On Customs 3 and Customs 17 as sources, see John J. McCusker, "The Current Value of English Exports, 1697 to 1800," *William and Mary Quarterly*, 3d ser., 28 (October 1971): 607–28.

Table 2. British Rice Imports by Area, 1831–1850
(clean rice equivalents)

	1831–40	1841–50
Total Imports (cwts)	3,723,027	8,312,825
Percentage of Total:		
Northern Europe	0.12%	0.18%
Southern Europe	0.12	0.62
North America	20.52	14.26
West Indies and Central America	0.00	0.09
South America	0.19	0.85
Africa	0.69	0.23
Asia	78.36	83.77
Total	100.00%	100.00%

SOURCES: [Great Britain] *House of Lords, Sessional Papers, Session 1842, Vol. 10*, pp. 312–13; *House of Lords, Sessional Papers, Session 1854–1855, Vol. 10*, pp. 97–99. Note that in converting into clean rice equivalents, I assumed that a bushel of rice “in the husk” weighed 45 to 50 pounds and that a quarter of rice “in the husk” weighed 400 pounds. In addition, I employed a conservative assumption that 100 pounds of rice “in the husk,” after milling, would make 60 pounds of clean rice.

completely shut off the flow of American rice to Europe. At roughly the same time, regional harvest failures and shortfalls in Europe—the shortfalls of 1795 and 1800 in England are cases in point—as well as broader forces related to industrialization, urbanization, commercialization, and population growth interacted to effect, if not institutionalize, a great increase in European demand for foodstuffs and industrial crops, including rice. Taken together, these forces help to explain such diverse developments as the termination in 1813 of the East India Company’s trade monopoly and the gradual demise in the first half of the nineteenth century of the English Corn Laws. More directly, they help to explain the growing European interest in Bengali foodstuffs and raw materials and a similar interest in a second East Indian supply source, the Dutch-controlled island of Java.¹⁹

¹⁹ See, for example, C. Northcote Parkinson, *Trade in the Eastern Seas 1793–1813* (Cambridge, 1937), 86; Holden Furber, *John Company at Work: A Study of European Expansion in India in the Late Eighteenth Century* (Cambridge, Mass., 1948); P. J. Marshall, *East Indian Fortunes: The British in Bengal in the Eighteenth Century* (Oxford, 1976), 29–50. On the supply disruptions occasioned by the American Revolution and the Napoleonic wars, see Customs 17/4–30, Customs Office Records, Public Record Office, London. The revolution wreaked havoc on the Lower South, bringing considerable damage and dislocation to the South Carolina–Georgia rice industry. See Jerome J. Nadelhaft, *The Disorders of War: The Revolution in South Carolina* (Orono, Me., 1981), 45–104; Benjamin Quarles, *The Negro in the American Revolution* (Chapel Hill, N.C., 1961), 158–81; Rachel N. Klein, *Unification of a Slave State: The Rise of the Planter Class in the South Carolina Backcountry, 1760–1808* (Chapel Hill, 1990), 114–15.

On the British harvest failures of 1795 and 1800, see W. Freeman Galpin, *The Grain Supply of England during the Napoleonic Period* (New York, 1925), 14, 20–21; Walter M. Stern, “The Bread Crisis in Britain, 1795–96,” *Economica*, n.s., 31 (May 1964): 168–87; Richard Perren, “Markets and Marketing,” in *The Agrarian History of England and Wales*, Vol. 6: 1750–1850, G. E. Mingay, ed. (Cambridge, 1989), 191–274, esp. 202. Note that England was a net importer of grain throughout the late eighteenth century. See Perren, “Markets and Marketing,” 205. On the shortage of provisions in Great Britain and Europe in 1795 and 1800, the demand for Indian rice, and the competitive advantage Indian rice enjoyed over Carolina rice, see Draft of letter from the Secret Committee to the Government of Bengal, July 7, 1795, in *Indian Records Series: Fort William–India House Correspondence*.

This island, the pearl of the Malay archipelago, had been trading directly with the West since it fell under Portuguese control in the sixteenth century, but its effective integration into the Western economy did not come about for several centuries thereafter. Once again, the British were involved in promoting this integration, whether through the Anglo-Dutch settlement after the American Revolution, which ended the V.O.C. trade monopoly in the Dutch East Indies, or through the economic and social reforms instituted by Sir Thomas Raffles between 1811 and 1816 when Java was under British rule. The establishment by the Dutch themselves of the so-called Preanger System in the eighteenth century and, more important, the *Cultuurstelsel* or Culture System in 1830, however, contributed even more to such integration. With the establishment of these systems, which essentially forced the indigenous population to produce cash crops, first, for the V.O.C. and, then, for the state, tropical commodities from the island such as coffee, sugar, and indigo began streaming into Amsterdam and, later, Rotterdam, thence to be distributed throughout Europe.²⁰

Rice originally was not included in either system—it was added under the *Cultuurstelsel* in 1843. But the general intensification of production and Western control both reflected and represented by these systems—and the creation of the N.H.M. (Nederlandsche Handel Maatschappij) or Dutch Trading Company in 1824—led to massive exports of this commodity to Europe as well, as illustrated in Table 3. Beginning in the late 1830s, so far as we can tell, Java rice alone rivaled, and often surpassed, U.S. rice in the main northern European entrepôts.²¹

For a variety of internal reasons—increasing demographic pressure on the island, the concomitant threat of food shortages, and the particular configuration

dence . . . , Vol. 17: 1792–1795, 137–39; David Scott to the marquis of Wellesley, September 30, 1800, and David Scott to William Pitt, December 24, 1800, in *The Correspondence of David Scott, Director and Chairman of the East India Company . . . 1787–1805*, C. H. Philips, ed., 2 vols. (London, 1951), 2: 287–88, 294–95; MacPherson, *Annals of Commerce*, 4: 362–63. One should note that very large quantities of East Indian rice were imported by Great Britain in 1796 and in 1802. See Customs 17/18, 24, Customs Office Records, Public Record Office.

²⁰ On the Preanger System and the Culture System, see, for example, J. S. Furnivall, *Netherlands India: A Study of Plural Economy* (Cambridge, 1939), 115–47; J. J. van Klaveren, *The Dutch Colonial System in the East Indies* (The Hague, 1953), 59–64, 115–31; Clifford Geertz, *Agricultural Involvement: The Process of Ecological Change in Indonesia* (Berkeley, Calif., 1963), 47–82; C. Fasseur, “The Cultivation System and Its Impact on the Dutch Colonial Economy and the Indigenous Society in Nineteenth-Century Java,” in C. A. Bayly and D. H. A. Kolff, eds., *Two Colonial Empires: Comparative Essays on the History of India and Indonesia in the Nineteenth Century* (Dordrecht, 1986), 137–54.

²¹ On the N.H.M., see van Klaveren, *Dutch Colonial System in the East Indies*, 108–14. Data compiled by G. F. de Bruijn Kops reveal that Javanese rice exports to northern Europe averaged 293,442 piculs annually between 1837 and 1839. Upon conversion to Western measures, this means that Java exported, on average, over 39.9 million pounds of (partially milled, cargo) rice annually to this area between 1837 and 1839. The United States, on the other hand, exported, on average, about 25.7 million pounds of clean rice equivalents to northern Europe in 1837 and 1838, with another 1 million pounds annually going to southern Europe. Under the conservative assumption that 1 pound of cargo rice at the time equaled 0.8 pounds of U.S. clean rice, we find that Java was already exporting more rice to northern Europe than the United States was sending to Europe as a whole. Moreover, Javanese exports to northern Europe increased in the 1840s and 1850s. See G. F. de Bruijn Kops, *Statistiek van Den Handel en de Scheepvaart op Java en Madura Sedert 1825*, 2 vols. (Batavia, 1857–59), 2: 176–80; *U.S. Senate Executive Documents*, 2d Session, 25th Congress, 1837–1838, No. 318; *U.S. Senate Executive Documents*, 3d Session, 25th Congress, 1838–1839, No. 342.

Indian rice also continued to make inroads into Great Britain's rice trade during this period. See Table 2.

Table 3. Rice Exports from Java and Madura to the West, 1826–1856
(pounds of clean rice equivalents)

Year	Netherlands	Other European Countries	America	Total
1825	—	189,312	—	189,312
1826	—	9,792	—	9,792
1827	3,503,469	172,992	—	3,676,461
1828	9,602,470	2,211,360	—	11,813,830
1829	8,639,808	716,448	561,408	9,917,664
1830	3,120,493	4,896	—	3,125,389
1831	3,250,944	473,280	—	3,724,224
1832	2,265,216	2,975,136	—	5,240,352
1833	6,033,939	4,547,514	—	10,581,453
1834	2,701,178	1,431,264	695,232	4,827,674
1835	1,418,208	2,870,688	71,808	4,360,704
1836	11,450,330*	4,981,408	—	16,431,738
1837	9,681,024	10,696,128	538,560	20,915,712
1838	7,388,445†	17,634,521	3,945,197	28,968,163
1839	19,650,912	30,733,410	4,264,307	54,648,629
1840	9,987,143	8,874,348	1,365,440	20,226,931
1841	6,414,588‡	5,704,493‡	1,946,867	14,065,948
1842	13,860,358	7,033,702	2,446,368	23,340,428
1843	27,614,310	15,389,893	326,400	43,330,603
1844	17,446,141	8,961,421‡	850,054	27,257,616
1845	9,564,087	2,123,885	—	11,687,972
1846	17,301,811‡	7,371,526	108,800	24,782,137
1847	27,115,136	9,217,318	293,760	36,626,214
1848	30,956,428	14,133,450	—	45,089,878
1849	25,891,244	12,647,347	—	38,538,591
1850	23,977,888	7,132,493	48,960	31,159,341
1851	34,611,376	7,548,544	1,464,992	43,624,912
1852	26,710,722	4,252,448	104,448	31,067,618
1853	12,424,633	7,752,870	1,931,115	22,108,618
1854	21,069,306	4,520,999	1,088,000	26,678,305
1855	33,581,948	15,382,470	228,480	49,192,898
1856	66,758,807	17,917,583	3,052,602	87,728,992

NOTES:

*includes 1,534 koyangs of paddy, which would make about 3,755,232 pounds of clean rice (assuming that 1 pound of paddy was equal to 0.6 pounds of clean rice)

†includes 60 piculs of paddy (4,896 pounds of clean rice)

‡excluding minute amounts of paddy

SOURCE: G. F. de Bruijn Kops, *Statistiek van Den Handel en de Scheepvaart op Java en Madura Sedert 1825*, 2 vols. (Batavia, 1857–59), 2: 176–78. Note that in converting Javanese measurement units into Western equivalents, I assumed that 1 koyang was equal to 30 piculs (except for 1828, when internal evidence suggested that koyangs of 28 piculs were being employed) and that 1 Batavian picul was equal to 136 pounds. Moreover, since the rice shipped from Java to the West during this period was (partially milled) cargo rice for the most part, I used a multiplier of 0.8 to transform cargo rice into clean rice equivalents. That is to say, I assumed that 1 pound of cargo rice was equal to 0.8 pounds of clean rice. On the weight of Batavian piculs during this period, see *Hunt's Merchants' Magazine*, 15 (September 1846): 328–29. On cargo rice and the derivation of the multiplier employed above, see Cheng Siok-Hwa, *The Rice Industry of Burma 1852–1940* (Singapore, 1968), 9–10 n. 24.

Almost all of the rice included in the “Other European Countries” category above went to northern Europe. Between 1826 and 1856, only 4,990 piculs (542,912 pounds of clean rice equivalents) were exported from Java and Madura to southern Europe. Finally, the heading “America” above refers to the entire Western Hemisphere.

of Javanese rural social structure—Java proved inconsistent in its role as an exporter of rice to the West.²² By the 1850s, however, nearby Lower Burma was being transformed by the British from a closed, underpopulated “natural” economy into what would soon become the greatest rice exporter in the world.²³

Burma had exported some rice prior to that time, particularly from Akyab in Arakan Province, but it was only after the Irrawaddy-Sittang Delta came under British rule in 1852 that Lower Burma’s revolutionary economic transformation began. The development of the Irrawaddy-Sittang Delta along with that of the Transbassac provinces of the Upper Mekong Delta in Cochinchina a few years later constitute two of the greatest episodes of rapid agricultural expansion in modern history. In the former case, Asian—primarily Indian Chettyar—and European capital collaborated with pioneering peasant cultivators to create a vast rice-exporting complex in a remarkably brief period of time. This economic transformation affected other parts of Lower Burma as well. In Arakan, for example, which had come under British control in 1826, rice exports boomed after 1852: between 1854–1855 and 1860–1861, an average of 107,252 tons of rice were exported to Europe annually from this province alone. This rice in all likelihood was partially milled “cargo rice” rather than clean rice, so the above figure must be reduced by about 20 percent to get an estimate of clean rice equivalents. Nonetheless, it is important to point out that the adjusted figure for Arakan alone—85,802 tons (171.6 million pounds)—far exceeded the average for total U.S. rice exports over the same seven-year period.²⁴

WHAT WAS IT ABOUT BENGAL, JAVA, AND LOWER BURMA in particular and about the Western economy as a whole that led both to the rise of the East Indies as a supply source and—as Tables 4 and 5 demonstrate—to the tepid growth and later stagnation of U.S. rice exports in the period between 1790 and the Civil War?

On one level, the answer is simple: Southeast Asia rose to prominence as a supply source for the West because it could outcompete other rice-producing areas. But why? This question is impossible to answer categorically, but I can offer some suggestive possibilities. As Marx pointed out long ago, “windfalls” often

²² See Furnivall, *Netherlands India*, 137–38 and *passim*; W. R. Hugenholtz, “Famine and Food Supply in Java 1830–1914,” in Bayly and Kolff, *Two Colonial Empires*, 155–88. Note that in the late nineteenth century, the orientation of the export trade of the Dutch East Indies shifted from Europe back to Asia. See A. J. H. Latham, *The International Economy and the Undeveloped World 1865–1914* (London, 1978), 92–93.

²³ See, for example, Cheng Siok-Hwa, *The Rice Industry of Burma 1852–1940* (Singapore, 1968), 1–15 and *passim*; Michael Adas, *The Burma Delta: Economic Development and Social Change on an Asian Rice Frontier, 1852–1941* (Madison, Wis., 1974), 15–57.

²⁴ See the works mentioned in note 23 above. Also see U Tun Wai, *Economic Development of Burma from 1800 till 1940* (Rangoon, 1961); Norman G. Owen, “The Rice Industry of Mainland Southeast Asia 1850–1914,” *Journal of the Siam Society*, 59 (July 1971): 75–143. Note that prior to about 1880, the Chettyars’ role in agricultural finance in Burma was largely indirect. After that date, however, the Chettyars increasingly moved into direct lending to Burmese cultivators themselves. See Michael Adas, “Immigrant Asians and the Economic Impact of European Imperialism: The Role of the South Indian Chettyars in British Burma,” *Journal of Asian Studies*, 33 (May 1974): 385–401. The figures on Arakan’s rice exports to Europe between 1854–1855 and 1860–1861 are from [H. R. Spearman], *British Burma Gazetteer*, 2 vols. (Rangoon, 1880), 1: 462.

Table 4. U.S. Rice Output, Exports, and Export/Output Ratios, 1819–1860 (pounds)

Year	Output (clean rice)	Exports (clean rice)	Export/Output Ratio (%)
1819	53,292,000	42,997,800	80.68
1820	69,354,600	52,932,600	76.32
1821	60,544,200	52,253,400	86.31
1822	61,951,200	60,819,000	98.17
1823	75,463,800	67,937,400	90.03
1824	66,133,200	58,209,000	88.02
1825	70,348,800	66,637,800	94.72
1826	79,686,600	80,110,800	100.53
1827	87,406,800	105,011,400	120.14
1828	92,355,600	102,981,600	111.51
1829	87,565,800	78,418,200	89.55
1830	81,351,600	69,910,200	85.94
1831	91,155,000	72,196,200	79.20
1832	106,953,000	86,497,800	80.87
1833	87,222,600	73,131,600	83.84
1834	90,600,000	66,510,600	73.41
1835	101,310,600	127,789,800	126.14
1836	90,574,800	63,650,400	70.27
1837	71,604,600	42,628,800	59.53
1838	81,949,800	55,992,000	68.32
1839	80,841,422	60,996,000	75.45
1840	84,252,600	60,970,200	72.37
1841	88,952,968	68,770,200	77.31
1842	94,007,484	64,059,600	68.14
1843	89,879,145	80,829,000	89.93
1844	111,759,000	71,172,600	63.68
1845	89,765,000	74,404,200	82.89
1846	97,741,500	86,656,200	88.66
1847	103,040,500	60,241,800	58.46
1848	119,199,500	77,316,600	64.86
1849	143,614,102	76,241,400	53.09
1850	102,775,800	63,354,000	61.64
1851	105,733,800	71,839,800	67.94
1852	102,467,400	40,624,200	39.65
1853	101,430,600	63,072,600	62.18
1854	70,872,000	39,421,600	55.62
1855	103,606,200	67,616,000	65.26
1856	99,564,600	68,322,800	68.62
1857	108,243,600	58,122,200	53.70
1858	116,293,200	77,070,400	66.27
1859	187,167,032	81,632,600	43.61
1860	105,279,200	43,512,400	41.33

SOURCE: U.S. Department of Agriculture, Bureau of Statistics, *Rice Crop of the United States 1712–1911*, by George K. Holmes, Circular No. 34 (Washington, D.C., 1912), 7–9. The figure above for output in 1849 differs from the figure given in the *Seventh Census of the United States, 1850* because the figure given for clean rice in the census is actually for rough rice. See J. D. B. De Bow, *Statistical View of the United States . . . Being a Compendium of the Seventh Census* (Washington, D.C., 1854), 170, 173–74. I would like to thank Robert E. Gallman for bringing this to my attention. In estimating clean rice production for 1849, I assumed that 45 pounds of rough rice were equal to 30 pounds of clean rice.

resulted from the effective linkage of pre-capitalist, non-capitalist, or “natural” economies to that of the West, and such a linkage did, in fact, occur in Bengal, Java, and Lower Burma in the nineteenth century. Land and labor in these areas

Table 5. U.S. Rice Exports by Destination, 1730–1739, 1790–1799, 1830–1839, and 1850–1859

	1730–39	1790–99	1830–39	1850–59
Total Exports (tons)	104,696	324,115	368,195	311,975
Destination (% of total)				
Northern Europe	77.4%	64.2%	53.8%	48.6%
Southern Europe	17.6	8.2	2.1	0.9
North America		0.7	1.8	1.9
West Indies and Central America	5.0	25.6	40.8	36.3
South America	0.0	0.0	1.1	11.3
Africa	0.0	0.9	0.2	0.5
Asia	0.0	0.1	0.1	0.4
Uncertain	0.0	0.3	0.0	0.0
Total	100.0	100.0	99.9	99.9

SOURCES: United States Department of Commerce, Bureau of the Census, *Historical Statistics of the United States: Colonial Times to 1970*, 2 vols. (Washington, D.C., 1975), 2: 1192; James Glen, *A Description of South Carolina . . .* (London, 1761), 93; Converse D. Clowse, *Measuring Charleston's Overseas Commerce, 1717–1767: Statistics from the Port's Naval List* (Washington, D.C., 1981), 59–64; *American State Papers . . .*, Class IV: Commerce and Navigation, Vol. 7 (Washington, D.C., 1832), 32, 122, 235, 284, 308, 339, 359, 381, 414, 428; U.S. House of Representatives Executive Documents, Second Session, 21st Congress, No. 209, Vol. 4; U.S. House of Representatives Executive Documents, First Session, 22d Congress, No. 220, Vol. 5; U.S. House of Representatives Executive Documents, Second Session, 22d Congress, No. 234, Vol. 2; U.S. House of Representatives Executive Documents, First Session, 23d Congress, No. 258, Vol. 5; U.S. House of Representatives Executive Documents, Second Session, 23d Congress, No. 275, Vol. 5; U.S. House of Representatives Executive Documents, First Session, 24th Congress, No. 291, Vol. 6; U.S. House of Representatives Executive Documents, Second Session, 24th Congress, No. 304, Vol. 4; U.S. House of Representatives Executive Documents, Second Session, 25th Congress, No. 330, Vol. 10; U.S. House of Representatives Executive Documents, Third Session, 25th Congress, No. 349, Vol. 6; U.S. House of Representatives Executive Documents, First Session, 26th Congress, No. 369, Vol. 7; U.S. House of Representatives Executive Documents, Second Session, 31st Congress, No. 604, Vol. 8; U.S. Senate Executive Documents, First Session, 32d Congress, No. 628, Vol. 16; U.S. Senate Executive Documents, Second Session, 32d Congress, No. 662, Vol. 5; U.S. Senate Executive Documents, First Session, 33d Congress, No. 703, Vol. 12, Part 2; U.S. Senate Executive Documents, Second Session, 33d Congress, No. 865, Vol. 16; U.S. Senate Executive Documents, Second Session, 33d Congress, No. 750, Vol. 5; U.S. House of Representatives Executive Documents, First Session, 34th Congress, No. 865, Vol. 16; U.S. Senate Executive Documents, Third Session, 34th Congress, No. 886, Vol. 13; U.S. Senate Executive Documents, First Session, 35th Congress, No. 931, Vol. 14; U.S. Senate Executive Documents, Second Session, 35th Congress, No. 989, Vol. 15; U.S. Senate Executive Documents, First Session, 36th Congress, No. 1034, Vol. 2.

Note that the broad geographic units listed in the table should be considered final rather than intermediate destinations for the most part. The figures for the 1730s required some manipulation. According to Glen, 6 percent of total rice exports was consumed in Great Britain, Ireland, and the British Plantations. Data in Clowse and Coclanis indicate that very little was consumed in Great Britain or Ireland during this period. Thus I estimated that 5 percent of total exports went to the British Plantations and 1 percent to Great Britain and Ireland. See Clowse, *Measuring Charleston's Overseas Commerce, 1717–1767*, 59–64; Peter A. Coclanis, "Bitter Harvest: The South Carolina Low Country in Historical Perspective," *Journal of Economic History*, 45 (June 1985): 251–59, esp. 254. Also note that in converting antebellum data into tons, I assumed that 1 tierce was equal to 600 pounds.

were amazingly cheap by Western standards; indeed, as in other cases of "articulation" between peasant and commercial economies, family labor in producing households was often completely unremunerated.²⁵ In agro-climatic

²⁵ See Karl Marx, *Capital*, Frederick Engels, ed., Samuel Moore and Edward Aveling, trans., 3 vols. (New York, 1967), 1: 750–74. On the "cheapness" of land and labor in Southeast Asia, see, for

terms, this area was almost perfectly suited for rice production, and the fertility of Southeast Asian rice paddies has long been marveled at. Although the reasons for such fertility are still incompletely understood, most researchers today believe that the nitrogen-fixing effects of symbiosis between certain life forms present in the area—ferns of the genus *Azolla* and the blue-green alga *Anabaena azollae*—are crucial.²⁶

Given these considerations, it is understandable that Westerners looked favorably on Southeast Asia as a source of supply. Such favor seems even more understandable in light of other factors: first, because the indigenous populations already were experienced and relatively efficient rice producers. Unlike the cases of cotton and tobacco, two other crops grown in both temperate and tropical countries, rice production in Southeast Asia was not necessarily marked by low relative productivity. As John Komlos and I have shown elsewhere, for example, the total factor productivity of Burmese peasant producers, measured in standard Cobb-Douglas form, was roughly on a par with that of slave laborers on South Carolina and Georgia rice plantations.²⁷ Even under stringent neoclassical assumptions, then, Burmese rice producers were competitive with U.S. producers in the nineteenth century. W. Arthur Lewis was probably correct in citing low productivity as the main reason for the inability of tropical producers to compete with the United States in cotton and tobacco. The stunning success of Southeast Asia in world rice markets—as Lewis himself noted—may well have been the exception that proves the rule.²⁸

Another factor—power—also came into play, which could only enhance both the relative competitiveness and the attractiveness of Southeast Asia vis-à-vis other potential supply sources, particularly in more developed parts of the world. In the nineteenth century, extra-economic suasion, that is to say, European political and military power, could be and often was projected forcefully in Southeast Asia,

example, C. Edwards Lester, *The Glory and the Shame of England*, 2 vols. in 1 (New York, 1850), 2: 27–52; (London) *The Times*, February 6, 1874; U.S. Congress, House of Representatives, Ways and Means Committee, *Statements to the Committee of Ways and Means on the Morrison Tariff Bill . . .* (Washington, D.C., 1886), 205–32, esp. 215; U.S. House of Representatives Miscellaneous Documents, First Session, 51st Congress, 1889–90, No. 2774, vol. 15, *Tariff Hearings*, 927–37. On the specific advantages gained from the use of unremunerated labor in Southeast Asia, see U.S. House of Representatives Documents, 142, Second Session, 60th Congress, 1908–1909, No. 5552, 3666; U.S. House of Representatives Documents, 129, Third Session, 62d Congress, 1912–13, No. 6495, 2704. The theoretical problem of “articulation” between competing modes of production has been studied most closely by Pierre Philippe Rey and Giovanni Arrighi. For an empirical demonstration, see Joseph C. Miller, “A Marginal Institution on the Margin of the Atlantic System: The Portuguese Southern Atlantic Slave Trade in the Eighteenth Century,” in Barbara L. Solow, ed., *Slavery and the Rise of the Atlantic System* (New York, 1991), 120–50.

²⁶ See, for example, M. Y. Nuttonson, *Rice Culture and Rice-Climate Relationships . . .* (Washington, D.C., 1965). On the nitrogen-fixing effects of symbiosis, see Mary J. F. Gregor, “Associations with Fungi and Other Lower Plants,” in Frans Verdoon, ed., *Manual of Pteridology* (The Hague, 1938), 141–58; A. W. Moore, “*Azolla*: Biology and Agronomic Significance,” *Botanical Review*, 35 (January–March 1969): 17–34; D. J. Hill, “The Role of *Anabaena* in the *Azolla*-*Anabaena* Symbiosis,” *New Phytologist*, 78 (May 1977): 611–16; A. F. Dyer, ed., *The Experimental Biology of Ferns* (London, 1979), 575–76; M. S. Swaminathan, “Rice,” *Scientific American*, 250 (January 1984): 80–85, 90–93.

²⁷ See Peter Coclanis and John Komlos, “Time in the Paddies: A Comparison of Rice Production in the Southeastern United States and Lower Burma in the Nineteenth Century,” *Social Science History*, 11 (Fall 1987): 343–54.

²⁸ W. Arthur Lewis, *Growth and Fluctuations 1870–1913* (London, 1978), 201–02 especially.

shoring up and in some cases shaping economic choices being made along the muddy banks of the Irrawaddy and on the terraces of Kedjawèn. The administrative, fiscal, and tariff innovations introduced into Southeast Asia by the European powers—innovations described at length in the rich work of James C. Scott—offer cases in point.²⁹

Other factors were at work as well. Technological and organizational improvements in transoceanic shipping along with the *Pax Britannica* lowered the cost of carriage and insurance, and the coincident development of Australia and California—development spurred by the discovery of gold in both areas between 1849 and 1851—assured that numerous vessels returning to Europe from the Pacific would be interested in a bulky backhaul like rice. The customs preferences accorded colonial produce added further encouragement, if any was needed, but with or without such preferences East Indian rice could undersell higher quality Western rice anywhere in Europe. Given the foregoing considerations, the prevailing uses made of rice at this time, and the groups for whom the cereal generally was intended, it is in no way surprising that the European market responded less to North American quality than to East Indian price.³⁰

So intense was this pressure from the East that, as early as the 1830s, rice from this area was being sold in Western producing regions such as Italy and even South Carolina for less than homegrown rice. In the 1830s and 1840s, some Western producers—Portugal and the United States, for example—raised tariffs on imported rice, in the case of Portugal, by roughly 1,000 percent. Although the United States was able to stave off East Indian competition for a time, the penetration of East Indian rice into Europe was not turned back. Rather, it accelerated sharply as time passed. As a result of both the dislocations in the U.S. industry occasioned by the Civil War and, perhaps more important, the decline in transportation costs occasioned by the rise of steam shipping and the opening of the Suez Canal, East Indian rice poured into Europe in the late nineteenth century, as illustrated by the figures in Tables 6 and 7. Exports to Europe from Lower Burma alone, for instance, averaged 682,000 tons of clean rice equivalents annually between 1881 and 1890, which figure exceeded total rice production in

²⁹ On the projection of Western power in Southeast Asia generally during this period, see, for example, D. G. E. Hall, *A History of South-East Asia*, 4th edn. (London, 1981), part 3, 497–742. On the coercive effects of Western administrative, fiscal, and tariff innovations in Southeast Asia, see James C. Scott, *The Moral Economy of the Peasant: Rebellion and Subsistence in Southeast Asia* (New Haven, Conn., 1976).

³⁰ On technological and organizational changes in transoceanic shipping, see Douglass C. North, "Ocean Freight Rates and Economic Development 1750–1913," *Journal of Economic History*, 18 (December 1958): 537–55; North, "Sources of Productivity Change in Ocean Shipping, 1600–1850," *Journal of Political Economy*, 76 (September–October 1968): 953–70. On the importance of the development of Australia and California for the Southeast Asian rice trade, see *Hunt's Merchants' Magazine*, 41 (October 1859): 445–48; 42 (January 1860): 62–66. On British tariff preferences in favor of imperial rice, for example, see the schedules reprinted in *Southern Agriculturalist*, 1 (October 1828): 459–61; *Hunt's Merchants' Magazine*, 7 (October 1842): 367–88, esp. 379; *Hunt's Merchants' Magazine*, 16 (April 1847): 405–08.

U.S. rice generally was considered by Westerners to be of higher quality than East Indian rice. See H. J. S. Cotton, "The Rice Trade of the World," *Calcutta Review*, 58 (1874): 267–302; Gray, *History of Agriculture*, 2: 725. On the price differentials between "Carolina" rice and East Indian rice in the first half of the nineteenth century, see the collection of "Prices Current" from Liverpool and Rotterdam in the Enoch Silsby Collection, Southern Historical Collection, University of North Carolina, Chapel Hill, N.C.

Table 6. Exports of Rice from Bengal to the West, 1861–1862 to 1872–1873 (tons)

<i>Year</i>	<i>Exports to the West</i>	<i>Total Foreign Exports</i>	<i>Exports to West as % of Total</i>
1861–62	129,655	341,198	38.00
1862–63	72,139	407,793	17.69
1863–64	53,345	388,814	13.72
1864–65	26,344	403,432	6.53
1865–66	32,559	255,167	12.76
1866–67*	24,054	160,357	15.40
1867–68	110,810	268,892	41.21
1868–69	57,485	254,244	22.61
1869–70	40,433	190,093	21.27
1870–71	65,588	244,916	26.78
1871–72	77,158	252,812	30.52
1872–73	89,687	355,054	25.26
	779,257	3,522,772	22.12

*eleven months

Disaggregated Exports to the West as Percent of Total Exports

<i>Year</i>	<i>United Kingdom</i>	<i>France</i>	<i>Germany</i>	<i>North and South America</i>	<i>West Indies</i>	<i>Western Total</i>
1861–62	24.09	4.56	2.04	5.46	1.85	38.00
1862–63	11.39	1.19	1.21	2.62	1.28	17.69
1863–64	9.39	0.70	0.35	2.12	1.16	13.72
1864–65	2.46	0.26	0.12	1.85	1.84	6.53
1865–66	8.40	0.15	—	0.47	3.74	12.76
1866–67*	7.89	0.30	—	—	7.21	15.40
1867–68	32.59	0.80	0.90	0.03	6.89	41.21
1868–69	16.57	0.56	—	0.73	4.75	22.61
1869–70	10.68	1.39	—	1.73	7.47	21.27
1870–71	18.43	0.65	0.18	—	7.52	26.78
1871–72	21.15	0.47	0.17	0.53	8.20	30.52
1872–73	15.61	0.02	—	1.49	8.14	25.26

*eleven months

SOURCE: H. J. S. Cotton, "The Rice Trade of the World," *Calcutta Review*, 58 (1874): 267–302, esp. 274–75. For detailed figures on Indian rice exports to the West between 1877 and 1915, see Sir William Wilson Hunter, *The Indian Empire: Its Peoples, History, and Products*, 3d edn. (London, 1893), 686; Imperial Institute, *Indian Trade Enquiry, Reports on Rice* (London, 1920), 8, 36–38.

the United States for the entire decade of the 1880s, or the decade of the 1850s, for that matter. Other transportation and communications improvements—steel-hull shipping, compound and triple-expansion steam engines, and the laying of submarine telegraph cable between Europe and Southeast Asia—the rise of other exporters such as Siam and Cochinchina, and the establishment of a rice futures market in Rangoon contributed further to the onslaught as the century drew to a close.³¹

³¹ *Southern Agriculturalist*, 11 (March 1838): 127–29; Armando Castro, "Orizicultura," in Joel Serrão, ed., *Dicionário de História de Portugal*, 3: 243–45; U.S. Bureau of Statistics, *Report of the Chief of the Bureau of Statistics on Customs-Tariff Legislation, Appendix A . . .*, prepared by A. W. Angerer (Washington, D.C., 1872), 72–73, 76–77, 82–83. Note, too, that in Italy duties on imported rice rose

Table 7. Mean Yearly Rice Exports from Burma, Siam, and Cochinchina, 1872–1881 to 1902–1911 (Thousands of Metric Tons)

	Burma		Siam	
	Asia	Other	Asia	Other
1872–1881	204	703	161	37
1882–1891	281	814	268	61
1892–1901	783	862	519	53
1902–1911	1,277	1,134	855	99

	Cochinchina		Totals		
	Asia	Other	Asia	Other	Total
1872–1881	292	23	657	763	1,420
1882–1891	459	37	1,008	912	1,920
1892–1901	482	164	1,784	1,079	2,863
1902–1911	572	221	2,704	1,454	4,158

SOURCE: Randolph Barker, *et al.*, *The Rice Economy of Asia*, 2 vols. (Washington, D.C., 1985), 1: 187. In order to correct for a minor computational error, I have adjusted Barker's total export figure for 1892–1901. The data above are based on material collected in Norman G. Owen, "The Rice Industry of Mainland Southeast Asia 1850–1914," *Journal of the Siam Society*, 59 (July 1971): 75–143, especially Table II-A.

Nor was Europe the only area swamped by East Indian rice in the late nineteenth century, a period in which commodity markets were being rapidly integrated throughout the world. Thus the United States, once the West's great rice exporter, became a major importer in this period and remained so until well into the twentieth century, as Table 8 demonstrates. This occurred despite increased duties imposed on imported rice. In other parts of the world, a similar pattern of rising imports from the East prevailed as well. One should note, too, that the rice trade within Asia itself grew substantially in the period, partly as a

substantially in the late nineteenth century. See Elda Gentili Zappi, *If Eight Hours Seem Too Few: Mobilization of Women Workers in the Italian Rice Fields* (Albany, N.Y., 1991), 6–7. On the decline in transport costs associated with steam shipping, allied technological improvements, and the opening of the Suez Canal, see Latham, *International Economy and the Undeveloped World*, 17–39; A. J. H. Latham, "From Competition to Constraint: The International Rice Trade in the Nineteenth and Twentieth Centuries," *Business and Economic History*, 2d ser., 17 (1988): 91–102; Daniel R. Headrick, *The Tentacles of Progress: Technology Transfer in the Age of Imperialism, 1850–1940* (New York, 1988), 25–31. The data for Burma and the United States are from Cheng, *Rice Industry of Burma, 1852–1940*, 9–15, 237–39, 257–59; Coclanis, *The Shadow of a Dream*, 133–42; U.S. Department of Agriculture, Bureau of Statistics, *Rice Crop of the United States, 1712–1911*, by George K. Holmes, Circular No. 34 (Washington, D.C., 1912), 3–11.

On Germany's massive rice imports from Asia in the second half of the nineteenth century and in the early twentieth century, see Paul Arndt, *Die Handelsbeziehungen Deutschlands zu England und den englischen Kolonien* (Berlin, 1899); Edwin J. Clapp, *The Navigable Rhine* (Boston, 1911), 33–37, 71–92; Ludwig Wendemuth and Walter Botcher, *The Port of Hamburg . . .*, Wilhelm Eggers, trans. (Hamburg, 1927), 222, 224, 226; Dieter Glade, *Bremen und der Ferne Osten* (Bremen, 1966), 35–39, 90–99; Hermann Kellenbenz, "German Trade Relations with the Indian Ocean from the End of the Eighteenth Century to 1870," *Journal of Southeast Asian Studies*, 13 (March 1982): 133–52; Kellenbenz, "Shipping and Trade between Hamburg-Bremen and the Indian Ocean, 1870–1914," *Journal of Southeast Asian Studies*, 13 (September 1982): 349–86.

Table 8. Rice Imports to and Exports from the United States, 1872–1911
(Millions of Pounds)

Year (ending June 30)	Imports		Exports	
	Rice	Rice Flour, Rice Meal, and Broken Rice ^a	Rice	Rice Bran, Meal, and Polish ^b
1872	74.6		0.4	
1873	83.8		0.3	
1874	73.3		0.6	
1875	59.4		0.3	
1876	71.6		0.4	
1877	64.0		1.3	
1878	47.5		0.6	
1879	75.8		0.7	
1880	57.0		0.2	
1881	68.7		0.2	
1882	79.4		0.1	
1883	96.7		0.1	
1884	106.6		0.2	
1885	81.1	38.0	0.2	
1886	60.1	37.5	0.3	
1887	56.0	47.9	0.6	
1888	100.8	54.8	0.4	
1889	132.2	54.1	0.4	
1890	68.4	55.7	0.4	
1891	133.1	81.3	0.3	
1892	85.1	63.0	^c	10.3
1893	81.1	66.5	0.8	13.0
1894	86.8	55.4	0.8	10.0
1895	141.3	78.3	0.1	1.5
1896	78.2	68.5	1.3 ^d	13.7
1897	133.9	63.9	0.4	3.5
1898	129.8	60.5	0.6	5.6
1899	153.8	50.3	0.9	14.5
1900	93.6	23.0	12.9	28.1
1901	74.6	42.6	1.1	24.4
1902	75.7	82.0	0.6	29.0
1903	78.3	91.3	0.5	19.2
1904	75.3	78.9	2.4	26.7
1905	43.4	63.1	74.9	38.4
1906	58.5	108.1	4.0	34.2
1907	71.3	138.3	2.4	27.7
1908	87.6	125.2	2.2	26.2
1909	88.8	134.1	1.6	18.9
1910	82.7	142.7	7.0	19.7
1911	76.7	132.1	15.6	14.5

NOTES: ^afirst stated 1885; ^bfirst stated 1892; ^cincluded in "Rice Bran, Meal, and Polish"; ^d896,000 pounds damaged

SOURCES: U.S. Department of the Treasury, *Statistical Abstract of the United States, 1881, Fourth Number* (New York, 1964), 74–75, 97; U.S. Department of the Treasury, *Statistical Abstract of the United States, 1891, Fourteenth Number* (New York, 1964), 131, 136, 158; U.S. Department of the Treasury, *Statistical Abstract of the United States, 1901, Twenty-Fourth Number* (Washington, D.C., 1902), 192, 220; U.S. Department of Commerce, *Statistical Abstract of the United States, 1911, Thirty-Fourth Number* (Washington, D.C., 1912), 421, 455. Note that after June 30, 1900, commerce with Puerto Rico and Hawaii is not included in the foreign commerce of the United States. George K. Holmes, however, includes these areas in his calculations of U.S. rice imports and exports between 1901 and 1911. See U.S. Department of Agriculture, Bureau of Statistics, *Rice Crop of the United States, 1712–1911*, by George K. Holmes, Circular 34 (Washington, D.C., 1912). Note, too, that the United States first becomes a net importer in 1861.

Table 9. Rice Production in the United States, 1839–1919
(Millions of Pounds of Clean Rice)

	1839	1849	1859	1869	1879	1889	1899	1909	1919
Total Production	80.8	143.6	187.2	73.6	110.1	128.6	250.3	658.4	1,065.2
Primary production states									
South Carolina	75.0%	74.3%	63.6%	43.9%	47.3%	23.6%	18.9%	2.5%	0.4%
Georgia	15.3	18.1	28.0	30.2	23.0	11.3	4.5	0.7	0.2
North Carolina	3.5	2.5	4.1	2.8	5.1	4.6	3.2	0.0	0.0
Louisiana	4.5	2.0	3.4	21.5	21.1	58.8	69.0	49.6	45.3
Texas	0.0	0.0	0.0	0.1	0.1	0.1	2.9	41.2	15.0
Arkansas	0.0	0.0	0.0	0.1	0.0	0.0	0.0	5.9	19.2
California	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	19.6

SOURCE: Peter A. Coclanis, "Economy and Society in the Early Modern South: Charleston and the Evolution of the South Carolina Low Country" (Ph.D. dissertation, Columbia University, 1984), 387–97. The above figure for clean rice in 1849 differs from the figure given in the *Seventh Census of the United States, 1850* because the figure given for clean rice in the census is actually for rough rice. See J. D. B. De Bow, *Statistical View of the United States . . . Being a Compendium of the Seventh Census* (Washington, D.C., 1854), 170, 173–74. For an explanation of the figure for 1849, see Table 4 sources.

result, ironically, of the need to feed laborers working mines and plantations established by Western interests in places such as Sumatra, Java, Malaya, and Ceylon.³²

THE ECONOMIC EFFECTS OF INTEGRATION DID NOT END ONCE ASIAN RICE passed through Western customs gates. In the United States, the reality of international competition underpinned and reinforced a geographic shift—see Table 9—in domestic rice production from the South Atlantic region to the Old Southwest, to Louisiana, Arkansas, and Texas specifically. Rice cultivation in South Carolina and Georgia was intensive in nature, and numerous problems militated against the successful continuation of this type of cultivation in the postbellum period. Given such problems—a list of which would include damage done to production facilities during the war, the reduction in the labor participation rate after Appomattox and the concomitant breakdown in labor discipline, the severe shortage of capital in the area, and the long-term declines in both soil fertility and total factor productivity along the coastal plain—it is apparent why production

³² See Coclanis, *Shadow of a Dream*, 136–38, 282–84; Latham and Neal, "International Market in Rice and Wheat"; Loren Brandt, *Commercialization and Agricultural Development: Central and Eastern China 1870–1937* (New York, 1989), 14–38. On U.S. rice duties in the late nineteenth century, see *Tariff Acts Passed by the Congress of the United States from 1789 to 1895 . . .*, compiled and indexed by William H. Michael and Pitman Pulsifer (Washington, D.C., 1896), 187, 215, 290, 342, 343, 426. Note that while U.S. duties on rice in the late nineteenth century were higher even in nominal terms than they had been prior to the Civil War, their real burden was even greater because of the deflationary monetary conditions prevalent in the United States over much of the late nineteenth century. See Robert A. McGuire, "Deflation-Induced Increases in Post-Civil War U.S. Tariffs," *Economic History Review*, 2d ser., 43 (November 1990): 633–45. On the demand for rice for Western-controlled mines and plantations in Asia, see Latham, *International Economy and the Undeveloped World*, 75–94.

shifted in relative terms toward the southwestern states, where many of these problems were less serious.³³

Not only was cheap, fertile, readily irrigable land available in Louisiana, Arkansas, and Texas, but, after the so-called rice revolution of the mid-1880s, more rational, extensive cultivation techniques were successfully implemented in this region. Such techniques, which were based on the employment of midwestern small-grain technology, rationalized production and allowed for the achievement (once again) of economies of scale in the U.S. rice industry. At the same time, the relative importance of scarce, unruly labor in the production process was reduced. Even though these techniques proved unworkable in South Carolina and Georgia, they clearly enabled producers in the Southwest—and, later, in California—to persevere, at times, even to thrive in the (rapidly expanding) domestic market despite competition from the Far East.³⁴ In essence, then, what occurred with the migration of the industry to the Old Southwest was a shift in U.S. rice supply, the main determinants of which were greater international competition, rising labor costs, and technological change.

One cannot stress too strongly that it is misleading to attribute the decline of the South Atlantic rice industry solely, or even primarily, to dislocations occasioned by the Civil War. For generations, American scholars have sought to explain the changing configuration of the domestic rice industry solely in such terms. By broadening the geographical and analytical context within which we view the industry, we can at once avoid the *post hoc* fallacy and lessen the parochialism and myopia that, with a few notable exceptions, has characterized scholarship in the field. If the freedman's cry "no more mud work" explains certain things about the evolution—and migration—of the U.S. rice industry in the nineteenth century, it does not explain everything.³⁵ U.S. rice exports had been stagnant since the 1790s, domestic producers were being edged out of the most important foreign markets by the 1830s, considerable quantities of rice were being shipped to the U.S. from Java by 1855, and U.S. interests had established the first steam-powered rice mill in Siam by 1858.³⁶ In other words, market expansion, elaboration, and integration, and not just Lincoln, swamp angels, and Sherman's

³³ Coclanis, *Shadow of a Dream*, 136–42, 280–87.

³⁴ See Coclanis, *Shadow of a Dream*, 136–42, 280–87; Arthur H. Cole, "The American Rice-Growing Industry: A Study of Comparative Advantage," *Quarterly Journal of Economics*, 41 (August 1927): 595–643; Pete Daniel, *Breaking the Land: The Transformation of Cotton, Tobacco, and Rice Cultures since 1880* (Urbana, Ill., 1985), 39–61, 215–36; Henry C. Dethloff, *A History of the American Rice Industry 1685–1985* (College Station, Tex., 1988), 63–194.

³⁵ See John Scott Strickland, "'No More Mud Work': The Struggle for the Control of Labor and Production in Low Country South Carolina, 1863–1880," in Walter J. Fraser, Jr., and Winfred B. Moore, Jr., eds., *The Southern Enigma: Essays on Race, Class, and Folk Culture* (Westport, Conn., 1983), 43–62. At this point, it is important to recall how severely the South Carolina-Georgia rice industry was damaged during the American Revolution. Not only were markets disrupted but lands were laid waste, and many slave laborers were lost. The market context was such at the time, however, that the industry had a chance to recover and to reclaim its position. See note 19.

³⁶ On shipments of rice from Java to the United States in 1855, see Prices Current Collection, Box 4, Folder "Java," Special Collections, Baker Library, Harvard University Graduate School of Business Administration, Boston, Mass. Note that considerable quantities of rice apparently were shipped from Manila to California during the 1850s as well, although such shipments do not appear in official U.S. records. See *Singapore Free Press*, March 5, 1857; February 18, 1858. On the steam-powered rice mill set up by U.S. interests in Siam, see James C. Ingram, *Economic Change in Thailand 1850–1970* (Stanford, Calif., 1971), 70.

General Order No. 15 shaped the U.S. rice industry in the nineteenth century. We already have looked at the expansion and elaboration of world rice markets. Let us now turn to the problem of integration.

PERHAPS THE BEST TEST OF MARKET INTEGRATION is the degree of price uniformity existing over a particular area at a given time. It is difficult, however, to speak with complete confidence about historical commodity prices anywhere in the world. International comparisons of such prices are more difficult still for a variety of reasons, including differential grading practices, fluctuating exchange rates, tiered pricing schemes, and complex and often bewildering tariff policies. Moreover, in the case of rice, we are not even altogether clear about the bases on which prices, historically, were determined, for we often lack systematic data on such matters as the local prices of other relevant commodities and the supply of shipping available at any given time in the major rice ports. This said, it seems reasonable to suggest from extant data that the basic tendency over time was toward uniformity in world rice prices and thus toward the creation of one integrated market. This tendency was uneven and incomplete even on the eve of World War I; but, over the course of the one hundred-year period after the so-called long eighteenth century ended in 1815, the basic pattern is unmistakable.³⁷

This point is illustrated in Figure 1, which plots export prices, converted into pounds sterling, for U.S., Burmese, and Bengali rice. Correlation analysis demonstrates the trend toward integration as well: a correlation relating U.S. export prices and Burmese export prices (for cargoes to arrive in London) reveals that the association between these prices, as measured by the Pearson correlation coefficient, is both positive and statistically significant for the 1868–1910 period. To be sure, an r of 0.5 is only moderately strong, but the results are highly significant. Other interesting results emerge from analysis of the subperiod data. In only one subperiod, encompassing the years between 1882 and 1895, was there a strong and statistically significant association between the two series. This was precisely the period in which the U.S. rice industry was being transformed by the “rice revolution” in southwestern Louisiana. Furthermore, while it appears at first glance that the correlation results are weak and insignificant for the 1896–1910 subperiod, such results may have more to do with currency fluctuations in the United States than with anything else. This is suggested in Figure 2, which plots real rice prices—nominal prices deflated by indexed wholesale prices—over time. In real terms, the trend in U.S. prices during the last subperiod seems consistent with the overall trend established in Figure 1.

What about the relationship, noted earlier, between rice and wheat? For much of the eighteenth and nineteenth centuries, rice prices in the West seemed to move in rough tandem with prices for wheat and other small grains. This pattern continued in the late nineteenth century; but rice, which had often been more expensive than rye, barley, oats, and even wheat in northern European markets,

³⁷ See the price data assembled in Cole, *Wholesale Commodity Prices*, vol. 2; Strauss and Bean, *Gross Farm Income*, 69–71; Latham and Neal, “International Market in Rice and Wheat,” 276–77.

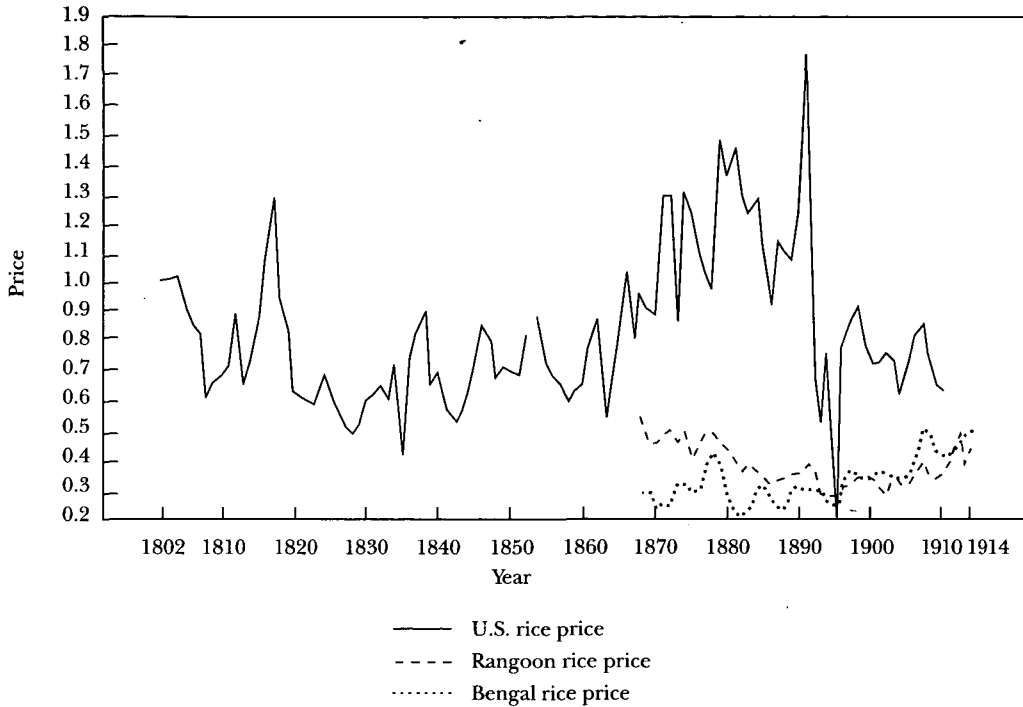


FIGURE 1: Rice Prices in Pounds Sterling (per cwt.)

SOURCES: U.S. Department of Agriculture, Bureau of Statistics, *Rice Crop of the United States, 1712–1911*, Circular 34, by George K. Holmes (Washington, D.C., 1912), 7–10; A. J. H. Latham and Larry Neal, “The International Market in Rice and Wheat, 1868–1914,” *Economic History Review*, 2d ser., 36 (May 1983): 260–80, esp. 276–77. In converting prices from dollars to sterling, I relied on series constructed by Lawrence H. Officer. See Officer, “Dollar-Sterling Mint Parity and Exchange Rates, 1791–1834,” *Journal of Economic History*, 43 (September 1983): 579–616; Officer, “Integration in the American Foreign-Exchange Market, 1791–1900,” *Journal of Economic History*, 45 (September 1985): 557–85. Note that in converting greenback prices to gold prices for the period between 1862 and 1878, I relied on Wesley C. Mitchell, *Gold, Prices and Wages under the Greenback Standard* (Berkeley, Calif., 1908), 4, Table 1.

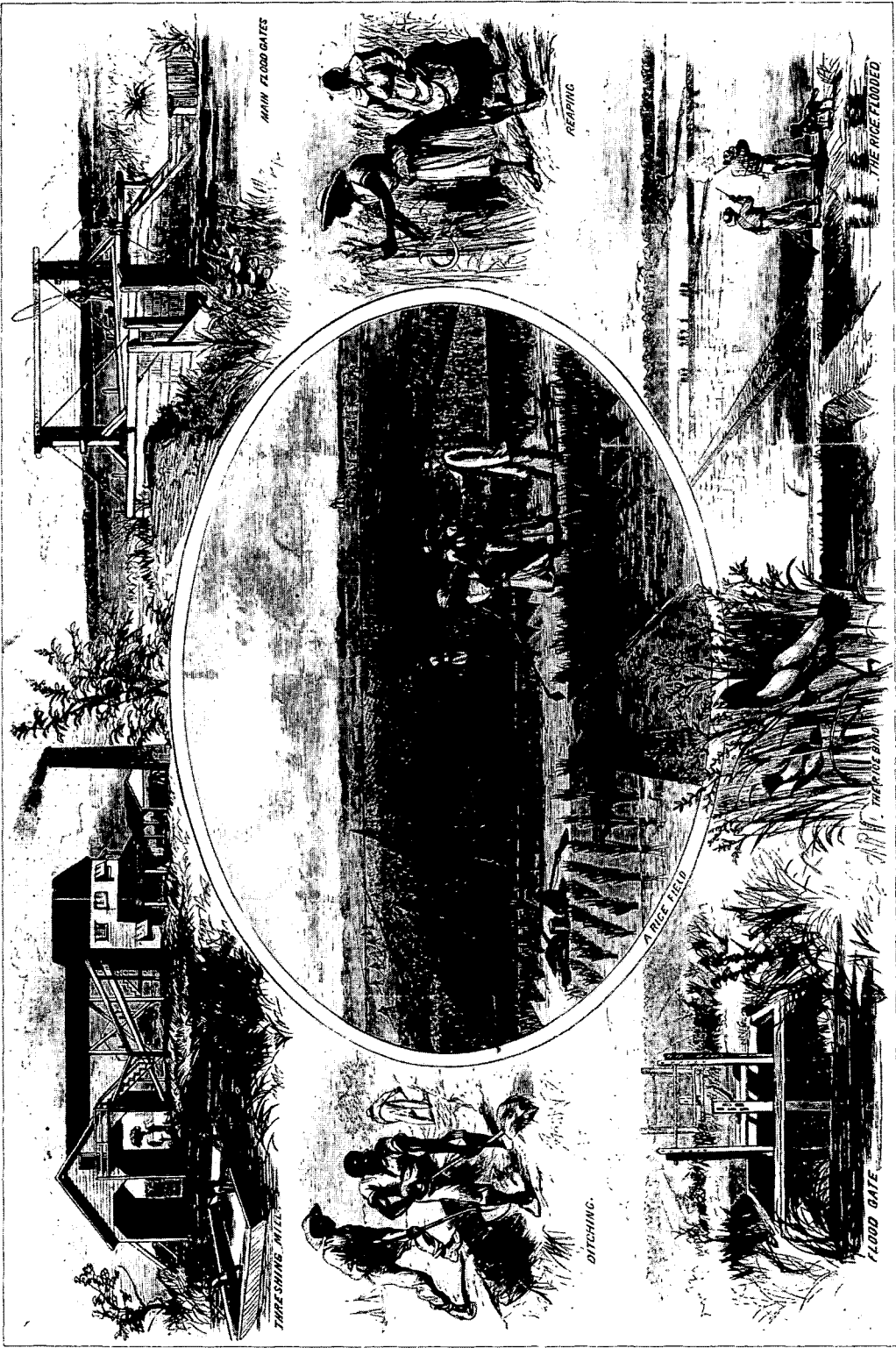
Correlation Between U.S. and Rangoon Rice Prices (Export Prices), 1868–1910

	Pearson Correlation Coefficients	Significance Levels
1868–1910	0.50561	0.0005
1868–1881	–0.37096	0.1916
1882–1895	0.78091	0.0010
1896–1910	0.22288	0.4246

generally became cheaper than these small grains, thanks largely to innovations in transportation and distribution and to the relative rise of shipments from the East.³⁸

One final point about price behavior in this increasingly integrated market for agricultural commodities: whereas some other agricultural commodities—cotton and sugar, for example—exhibited extreme price volatility in the West during this

³⁸ See the works mentioned in note 8, especially Latham and Neal, “International Market in Rice and Wheat.”



"Rice Culture on the Ogeechee [River], near Savannah, Georgia.—Sketched by A. R. Waud." From *Harper's Weekly*, January 5, 1867. The sketches are labeled, clockwise, "main flood gates," "reaping," "the rice flooded," "flood gate," "ditching," and "threshing mill." In the center is "a rice field."

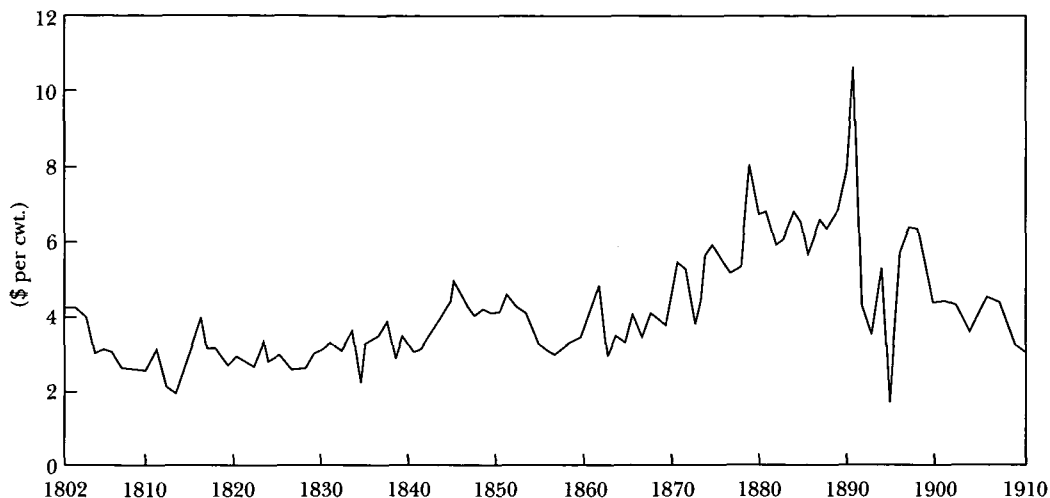


FIGURE 2: The Real Price of U.S. Rice, 1802–1910 (1910–1914 Dollars)

SOURCES: U.S. Department of Agriculture, Bureau of Statistics, *Rice Crop of the United States, 1712–1911*, Circular 34, by George K. Holmes (Washington, D.C., 1912), 7–10; U.S. Department of Commerce, Bureau of the Census, *Historical Statistics of the United States: Colonial Times to 1970*, 2 vols. (Washington, D.C., 1975), 1: 200–02. Note that in employing the Warren-Pearson and BLS wholesale price indexes, I reindexed the latter to the same base as the former: 1910–1914 = 100.

period, rice prices generally remained low and displayed less volatility. This was probably due not only to the fact that rice had numerous substitutes in many areas but to the fact that the supply of rice from Southeast Asia was sufficiently elastic to dampen the possible effects of any changes in demand.³⁹

The geographic shift of the U.S. rice industry, then, can be attributed in large part to the expansion, elaboration, and integration of the world rice market. But the evolution of this market had other effects as well, some of which we are still unable to appreciate in full. On the demand side, for example, rice helped at once to ensure and enrich the lives of Western consumers, as we have already seen. Whether one focuses on its role as a source of complex carbohydrates for the poor or of cellulose for industrial applications, the importance of rice is apparent.⁴⁰

As for supply, we find that increased market integration produced a strange and somewhat ironic variant of Gunnar Myrdal's "backwash effect." Just as such integration had crushed the Indian (handicraft) textile industry, it wreaked havoc on the South Atlantic rice industry, an industry that had made that region extremely wealthy when world markets had been less integrated in the eighteenth and early nineteenth centuries. Thus this region, suited for little else but rice, experienced relative decline in the antebellum period and absolute economic decline at least between 1860 and 1880 and probably even longer. By 1910, the region's rice industry had collapsed, and the South Carolina–Georgia Low

³⁹ See the works cited in note 37 above. Also see Gray, *History of Agriculture*, 2: 1027–34; U.S. Department of Commerce, Bureau of the Census, *Historical Statistics of the United States: Colonial Times to 1970*, 2 vols. (Washington, D.C., 1975), 1: 516–17. On prices for cotton and sugar, see, for example, Strauss and Bean, *Gross Farm Income*, 63–65; Noel Deerr, *The History of Sugar*, 2 vols. (London, 1949–50), 2: 530–31.

⁴⁰ On the roles and uses of rice, see above.

Country had become one of the poorest parts of the poorest census region in the United States. It has remained uncommonly poor by the standards of the developed world for most of the twentieth century.⁴¹

Elsewhere in the West, the Italian rice industry, bolstered by unification and other institutional changes, not only survived the challenge of integration but grew substantially between 1861 and World War I. Recent work suggests that northern Italy's agricultural sector as a whole performed fairly well during this period, especially after 1896, contributing to the area's industrialization. By this time, however, rice exports from Italy and from other Western producing areas such as Spain were tiny in comparison to those from the East.⁴² Let us therefore turn to the main centers of exportation.

Here the record is mixed and more complicated than either neoclassical or neo-Marxist models can readily accommodate. Even in light of the recent spate of revisionist works on South and Southeast Asia, few would argue that the position of peasant producers in Bengal or Java improved dramatically in the short run with the integration of world rice markets. Limited data suggest, however, that per capita income in these areas did rise somewhat after integration, and this rise can plausibly be explained in terms of either vent-for-surplus models or the standard theory of comparative advantage.⁴³ It seems likely, moreover, that the British-inspired transformation of the Burmese delta from a frontier into the greatest rice-exporting area in the world also raised the incomes of Burmese cultivators, at least between roughly 1855 and 1900. After that time, things began to change, and by the 1920s most of the income gains had been taken back. But that is another story, because, by that time, the closing of this frontier, problems relating to tenancy and agricultural credit, and Western developments in mechanization and genetic plant-breeding had begun to affect comparative advantage in new and profound ways.⁴⁴ The Burmese scenario seems to hold true, more or less, for Siam and Indochina as well, even though in Siam tenancy problems were

⁴¹ On "backwash effects," see Gunnar Myrdal, *Rich Lands and Poor: The Road to World Prosperity* (New York, 1957), 23–28. On the legacy of the rice regime in the South Carolina Low Country, see Coclanis, *Shadow of a Dream*, 141–60, 286–302.

⁴² See Barbiero, "Reassessment of Agricultural Production in Italy." For statistics on Italian rice production and exports between 1861 and 1920, see Istituto Centrale di Statistica (ISTAT), *Sommario di Statistiche Storiche dell'Italia 1861–1975* (Rome, 1976), 76, 118. On unification and other institutional changes, see Toniolo, *Economic History of Liberal Italy 1850–1918*, 48–59.

⁴³ For a relatively balanced assessment of the impact of market integration on Bengal, see Tripathi, *Trade and Finance in the Bengal Presidency 1793–1833*. Note that C. Fasseur—to cite but one of the Culture System revisionists—argues that market integration had certain positive effects on Java. See Fasseur, "Cultivation System." For a rough attempt to estimate per capita income in Southeast Asia between 1860 and 1913, see L. J. Zimmerman, "The Distribution of World Income 1860–1960," in Egbert de Vries, ed., *Essays on Unbalanced Growth: A Century of Disparity and Convergence* (S-Gravenhage, 1962), 28–55. According to Zimmerman, per capita income in this area (using 1953 dollars) grew from about \$48 in 1860 to about \$65 by 1913; see page 53. Also see Angus Maddison, *Dynamic Forces in Capitalist Development: A Long-Run Comparative View* (New York, 1991), 24–25; Ann Booth, "The Economic Development of Southeast Asia: 1870–1985," *Australian Economic History Review*, 31 (March 1991): 20–52. For a discussion of the rice trade's effects on Singapore, Southeast Asia's regional entrepôt for the trade, see W. G. Huff, "Bookkeeping Barter, Money, Credit, and Singapore's International Rice Trade, 1870–1939," *Explorations in Economic History*, 2d ser., 26 (April 1989): 161–89.

⁴⁴ On Burma, see Cheng, *Rice Industry of Burma, 1852–1940*, *passim*. On shifting comparative advantage in rice and the advent of the modern rice industry, see Cole, "American Rice-Growing Industry."

never as severe as they became in Burma, and in Indochina peasants exported not only rice but also considerable quantities of copra and maize.⁴⁵

AS SHOULD BE OBVIOUS BY NOW, historians still lack information vital for a complete understanding of the effects of market integration on Southeast Asia. The effects resulting from the intersection in the late nineteenth century of gold-based Western economies and silver-based Eastern economies, for example, have not yet been adequately explored. Few have followed up Jonathan Levin's important point about the distribution of income gains derived from the export sectors of Southeast Asian economies: how many of these gains went to what he calls "foreign factors" and "luxury importers" rather than to the peasant cultivators themselves? Indeed, we are not even sure about the mechanisms by which peasants entered into market relations. Did they willingly embrace them, or were they unwittingly ensnared? The still-unresolved Scott-Popkin controversy on peasant attitudes comes readily to mind in this regard. Clearly, there is much more to learn about the origins and early effects of integration. I hope at least to have initiated an instructive discussion.⁴⁶

How, then, do we end an introduction to the international history of the Western rice trade? We could, for example, close with a vignette intended to underscore the importance of the Arabs' early transfer of *O. sativa* to the West. Or with one about the so-called seed from Madagascar—sent by the treasurer of the English East India Company—that allegedly begat the North American rice industry. Or even with one about *japonica* rices and their significance in the Old Southwest in the twentieth century. Each of these alternatives would illustrate the interrelationship between East and West, as in a vague but powerful way do the epigraphs with which I began.⁴⁷

As a student of the South Atlantic rice industry primarily, I prefer, however, to end by alluding to war, for as every schoolchild knows, nothing hurt the competitive position of South Carolina and Georgia rice planters in the mid-nineteenth century as much as war and the aftermath of war. I will conclude,

⁴⁵ See Owen, "Rice Industry of Mainland Southeast Asia"; Ingram, *Economic Change in Thailand 1850–1970*; James C. Ingram, "Thailand's Rice Trade and the Allocation of Resources," in C. D. Cowan, ed., *The Economic Development of South-East Asia: Studies in Economic History and Political Economy* (London, 1964), 102–26; David B. Johnston, "Rice Cultivation in Thailand: The Development of an Export Economy by Indigenous Capital and Labor," *Modern Asian Studies*, 15 (February 1981): 107–26; Latham, *International Economy and the Undeveloped World*, 155–56.

⁴⁶ See Latham, *International Economy and the Undeveloped World*, 41–64, 65–101; Jonathan V. Levin, *The Export Economies: Their Pattern of Development in Historical Perspective* (Cambridge, Mass., 1960). Among the few scholars who have worked on income distribution (in Lower Burma) during the rice boom are Aye Hlaing and Michael Adas. See Aye Hlaing, "Trends of Economic Growth and Income Distribution in Burma, 1870–1940," *Journal of the Burma Research Society*, 47 (June 1964): 89–148; Adas, *Burma Delta*, 69–82, 141–53. The Scott-Popkin controversy has blazed in several of the social sciences for over a decade now. For the original positions, see Scott, *Moral Economy of the Peasant*; Samuel L. Popkin, *The Rational Peasant: The Political Economy of Rural Society in Vietnam* (Berkeley, Calif., 1979).

⁴⁷ Andrew M. Watson, *Agricultural Innovation in the Early Islamic World: The Diffusion of Crops and Farming Techniques, 700–1100* (Cambridge, 1983), 15–19; William Foster, *The East India House: Its History and Associations* (London, 1924), 113–24; Grist, *Rice*, 7, 94–97; Dethloff, *History of the American Rice Industry*, 78, 91.

then, with a passing reference to the Second Anglo-Burmese War (1852), which in opening up the Burma Delta to the West changed the course of southern history.⁴⁸ As every schoolchild knows.

⁴⁸ On this war and its background, see Hall, *History of South-East Asia*, 625–58.

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The American Revolutionaries, the Political Economy of Aristocracy, and the American Concept of the Distribution of Wealth, 1765–1900

JAMES L. HUSTON

“FOR THIS PURPOSE [maintaining republicanism] it will be highly politic, in every free state,” said Connecticut’s Benjamin Trumbull in 1773, “to keep property as equally divided among the inhabitants as possible, and not to suffer a few persons to amass all the riches and wealth of a country.”¹ In the late eighteenth century and throughout most of the nineteenth, American politicians and publicists averred, as Trumbull had in the heat of revolutionary ardor, that a republican form of government required an equitable (almost never an equal) distribution of wealth. Beyond the assertion, however, contemporaries offered few explanations about what forces created or distorted an equitable distribution of wealth.² Except for a few treatises in the nineteenth century, American writers, orators, and statesmen seemed content merely to reiterate the phrase, in the fashion of a sorcerer’s incantation, that a republic required an equal distribution of wealth among its citizenry. They did not detail the ways in which a republican distribution became established or specify the policies by which such a condition could be sustained. Exhortations about the need for a healthy distribution of wealth abounded, but a sustained analysis was rare indeed.

Yet Americans between 1776 and 1900 did possess a fairly coherent though imprecise concept of the distribution of wealth. It originated during the revolu-

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¹ Benjamin Trumbull, *Discourse, Delivered at the Anniversary Meeting of the Freemen of the Town of New-Haven, April 12, 1773* (New-Haven, 1773), 30–31. See also Ruth Bogin, *Abraham Clark and the Quest for Equality in the Revolutionary Era, 1774–1794* (East Brunswick, N.J., 1982), 36.

² Historians have frequently noted, with varying degrees of surprise, delight, and puzzlement, the emphasis revolutionaries gave the distribution of wealth: J. G. A. Pocock, *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition* (Princeton, N.J., 1975), 384–87; Gordon S. Wood, *The Creation of the American Republic, 1776–1787* (Chapel Hill, N.C., 1969), 72; Lance Banning, *The Jeffersonian Persuasion: Evolution of a Party Ideology* (Ithaca, N.Y., 1978), 30–31, 204–05; Steven Watts, *The Republic Reborn: War and the Making of Liberal America, 1790–1820* (Baltimore, Md., 1987), 9, 31–38, 73; Drew R. McCoy, *The Elusive Republic: Political Economy in Jeffersonian America* (Chapel Hill, 1980), 67–68. Two works that deal with the question of the popular or elitist understanding of wealth distribution and economic inequalities in the eighteenth and nineteenth centuries, but not as completely as one would like, are Peter D. McClelland, *The American Search for Economic Justice* (Cambridge, 1990), chap. 2; and J. R. Pole, *The Pursuit of Equality in American History* (Berkeley, Calif., 1978), chap. 5.

tionary era, when the movement's leaders established their understanding of the principles governing wealth distribution. Their theory—intimated in their protests over imperial regulation but never explicitly delineated as a separate topic—was composed of four axioms: the labor theory of property or value, the political economy of aristocracy, the laws of primogeniture and entail, and the population-to-land ratio. Nineteenth-century Americans, with some dissent, not only accepted the views of their revolutionary forebears but generally applied the axioms more rigidly than had the revolutionary generation itself. For nearly 130 years, Americans invoked the four axioms of the revolutionary leaders to explain wealth distribution and the redistributive tendencies of legislative proposals. Then the concept came to a sudden demise: the appearance of the large-scale corporation in the last two decades of the nineteenth century demolished the revolutionary explanation of the distribution of wealth and left society lurching frantically about in an effort to construct a new understanding. Part of the legacy of that transformation, which has permanently affected the perspective of modern historians, has been to make nearly impenetrable the economic reasoning of the citizens of the early republic.

The revolutionaries' concern over the distribution of wealth was prompted by a tenet in the broad and vague political philosophy of republicanism.³ In contrast to nations in which monarchs and aristocrats dominated the state, republics embodied the ideal of equality among citizens in political affairs, the equality taking the form of citizen participation in the election of officials who formulated the laws. Drawing largely on the work of seventeenth-century republican theorist James Harrington, Americans believed that if property were concentrated in the hands of a few in a republic, those few would use their wealth to control other citizens, seize political power, and warp the republic into an oligarchy. Thus to avoid descent into despotism or oligarchy, republics had to possess an equitable distribution of wealth.

The foundation, the bedrock, of the American theory of the distribution of wealth was a labor theory of property or, more generally, a labor theory of value. By the middle and late eighteenth century, the labor theory of property or value swamped nearly all aspects of Anglo-American economic and political discourse, drawing its strength from Protestantism's stress on reward being earned by the sweat of one's brow, from the regnant political formulation of John Locke that property rights originated in and were justified by the act of individual labor, and from eighteenth-century political economists and philosophers (such as Richard Petty, David Hume, Sir James Steuart, Francis Hutcheson, Lord Kames, and Adam Smith) who insisted that only labor created value. For the Americans, the

³ Standard works on American republicanism in the revolutionary era include Pocock, *Machiavellian Moment*, chaps. 14–15; Wood, *Creation of the American Republic*; Banning, *Jeffersonian Persuasion*; Robert E. Shalhope, "Toward a Republican Synthesis: The Emergence of an Understanding of Republicanism in American Historiography," *William and Mary Quarterly*, 3d ser., 29 (January 1972): 49–80; Shalhope, "Republicanism and Early American Historiography," *William and Mary Quarterly*, 39 (April 1982): 334–56. One of the many critiques of interpretations based on republicanism may be found in Joyce Appleby, *Capitalism and a New Social Order: The Republican Vision of the 1790s* (New York, 1984).

vital concept was that only an individual's labor created property, and therefore the individual had sole right to possession and disposition of that property.⁴

The labor theory of property or value dominated and controlled the American revolutionaries' understanding of wealth distribution. When the laborer obtained all the value or property that his⁵ labor produced—no more and no less—then the laborer was appropriately rewarded. In a society in which all members received the values their individual labor created, the resulting distribution of property or wealth was a just distribution; indeed, it was a natural distribution. The distribution was unlikely to be an equal one, for different people had different talents and capabilities, but the distribution would be equitable.

In eighteenth-century American parlance, the idea of the labor theory of property or value was expressed in the phrase "the fruits of labor." The fruits of labor were considered property. Injustice was committed whenever the laborer was deprived of the just "fruits of his labor." The phrase "the fruits of labor" was omnipresent in American political rhetoric throughout the nineteenth century and thus was one of the most important conceptual behests of the revolution.⁶

Throughout the revolutionary period, the leadership constantly reiterated the theme that property was the just reward of those who toiled. Josiah Quincy wrote in 1768 that "by the sweat of our brow, we earn the little we possess." Samuel Adams argued in the same year that "it is acknowledged to be an unalterable law

⁴ Americans in the eighteenth and nineteenth centuries, except for certain groups, adhered to a general labor theory of property or value, not the specific one later advanced by Karl Marx. Americans had a broad definition of "labor," whereas the Marxian one was physical, not mental, labor. For the twists and turns in theory, see Ronald L. Meek, *Studies in the Labour Theory of Value* (New York, 1956), 21–22, 64–78, 87–112, 165–78. On the theory in general use in America, see E. A. J. Johnson, *American Economic Thought in the Seventeenth Century* (1932; New York, 1961), 66–67, 71, 101, 104–05; Edmund S. Morgan, *The Challenge of the American Revolution* (New York, 1976), 91–93, 102; Louis B. Wright, *The Cultural Life of the American Colonies, 1607–1763* (New York, 1957), 23; Paul W. Conner, *Poor Richard's Politics: Benjamin Franklin and His New American Order* (New York, 1965), 41–42; Garry Wills, *Inventing America: Jefferson's Declaration of Independence* (Garden City, N.Y., 1978), 230–32; Helen Hill Miller, *George Mason: Gentleman Revolutionary* (Chapel Hill, N.C., 1975), 152–53; Donald Winch, *Adam Smith's Politics: An Essay in Historiographic Revision* (Cambridge, 1978), 18–19, 49; S. R. Sen, *The Economics of Sir James Stewart* (Cambridge, Mass., 1957), 51; John Locke, *Two Treatises of Government* [1690], Peter Laslett, ed., student edn. (Cambridge, 1988), 287–88 [second treatise, para. 27]. The notion of labor as a communal activity rather than one indicative of possessive individualism has been promoted by J. E. Crowley, *This Sheba, Self: The Conceptualization of Economic Life in Eighteenth-Century America* (Baltimore, Md., 1974), 50–72.

⁵ The analysis of the distribution of wealth in the late colonial and early national period was conducted almost entirely in masculine terms; not until the mid-nineteenth century or with labor protests did the labor theory of property or value indicate that the laborer deserved the fruits of his or her labor. Because the contemporary reference was always to the "fruits of his labor," that terminology will be kept in this article.

⁶ Historians have emphasized such words as sovereignty, virtue, and civic participation but for some reason have not recognized the importance of the phrase "the fruits of labor" for Americans in the eighteenth and nineteenth centuries. In the American case, the idea of the "fruits of labor" was the economic foundation of American republicanism, a feature that few authors of republicanism have acknowledged. The exception is William B. Scott, *In Pursuit of Happiness: American Conceptions of Property from the Seventeenth to the Twentieth Century* (Bloomington, Ind., 1977), 10, 16, 29–31, 37, 40–41. Gordon Wood has written recently that eighteenth-century colonials saw labor as degraded and had not celebrated it as the source of productivity and wealth, as they would during and after the revolution. While the desire among some of the colonial elite to achieve aristocratic status was probably present, I would argue that Americans by the middle of the eighteenth century saw honest labor and its fruits as a standard of equity, not a matter of wealth-bringing productivity. Gordon S. Wood, *The Radicalism of the American Revolution* (New York, 1992), 36–38, 136, 170–71, 186. See also Max Savelle, *Seeds of Liberty: The Genesis of the American Mind* (New York, 1948), 219–29, 243.

in nature, that a man should have the free use and sole disposal of the fruit of his honest industry, subject to no controul." The North Carolina Assembly in 1775 declared that they be granted the freedom "to enjoy the fruits of our own honest industry; to call that our own which we earn with the labor of our hands, and the sweat of our brows." In his *Letters of a Pennsylvania Farmer*, John Dickinson, in one of several places, proclaimed, "for as long as the *products* of our *labor*, and the *rewards* of our *care*, can properly be called *our own*, so long it will be worth one's while to be *industrious* and *frugal*."⁷ Benjamin Franklin while in London in 1770 attacked parliamentary taxation in this manner: "Three Millions, out of Fifteen of which his Majesty is Sovereign, are declared no longer Masters of the Fruits of their own Industry."⁸

By the end of the revolution, political groups of all shades had absorbed and celebrated the labor theory of property or value by reference to the "fruits of labor." Federalist Noah Webster, Equal Rights radical Thomas Paine, free trader Pelatiah Webster, historians Jeremy Belknap and David Ramsey, and agrarian radicals John Taylor of Caroline and George Logan all emphasized as a vital principle of the revolution the importance of securing to the laborer the fruits of his industry.⁹ No individual more succinctly captured the importance of the idea of the fruits of labor to the revolutionaries than the Connecticut Congregational minister Ezra Stiles. Offering a sermon to the Connecticut legislature upon the end of the war, Stiles distinctly characterized the American Revolution as the breaking of chains fastened on labor and the American republic as a nation devoted to the freedom of labor: "The enjoyment of this [civil and religious liberty], with property, has filled the english settlers in america with a most

⁷ Josiah Quincy in the *Boston Gazette*, October 3, 1768, quoted from Josiah Quincy, *Memoir of the Life of Josiah Quincy Jun. of Massachusetts* (1825; New York, 1971), 23; House of Representatives of Massachusetts to Henry Seymour Conway, February 13, 1768, in Harry Alonzo Cushing, ed., *The Writings of Samuel Adams*, 4 vols. (1904; New York, 1968), 1: 190; "Address of the Provincial Congress of North Carolina to the British Empire," September 3, 1775, in Hezekiah Niles, ed., *Principles and Acts of the Revolution in America*, revised by Samuel V. Niles (1822; New York, 1876), 316; John Dickinson, *Letters of a Farmer in Pennsylvania, to the Inhabitants of the British Colonies* [1768], in Paul Leicester Ford, ed., *The Political Writings of John Dickinson, 1764-1774* (1895; New York, 1970), 401. The "fruits of labor" theme was also strong in the writings of John Trenchard and Thomas Gordon; *Cato's Letters*, no. 62, January 20, 1721, in David L. Jacobson, ed., *The English Libertarian Heritage: From the Writings of John Trenchard and Thomas Gordon in "The Independent Whig" and "Cato's Letters"* (Indianapolis, Ind., 1965), 127-31.

⁸ [Benjamin Franklin], "The Colonist's Advocate: I," January 4, 1770, in Leonard W. Labaree and William B. Willcox, eds., *The Papers of Benjamin Franklin*, 28 vols. (New Haven, Conn., 1959-), 17: 7.

⁹ Noah Webster, Jr., *A Collection of Essays and Fugitiv Writings on Moral, Historical, Political and Literary Subjects* (Boston, 1790), 328; Thomas Paine, "A Serious Address to the People of Pennsylvania on the Present Situation of Their Affairs" [December 1778], in Philip S. Foner, ed., *The Complete Writings of Thomas Paine*, 2 vols. (New York, 1945), 2: 282; A Citizen of Philadelphia [Pelatiah Webster], *An Essay on Free Trade and Finance, humbly offered to the Consideration of the Public* (Philadelphia, 1779), 5; Jeremy Belknap, *The History of New-Hampshire*, 3 vols. (Boston, 1792), 3: 256-59; David Ramsey, *The History of the Revolution of South-Carolina, From a British Province to an Independent State*, 2 vols. (Trenton, N.J., 1785), 1: 7, 10; John Taylor, *An Enquiry into the Principles and Tendency of Certain Public Measures* (Philadelphia, 1794), 8-9, 21, 30; An American Farmer [George Logan], *Letters Addressed to the Yeomanry of the United States . . .* (Philadelphia, 1791), 5, 27-28. It is interesting that Tom Paine realized a limitation to the fruits of labor argument. In *Agrarian Justice* (1796), he noted that individual effort was not all that was involved in creating value: "Personal property is the *effect of society*; and it is impossible for an individual to acquire personal property without the aid of society, as it is for him to make land originally"; in Foner, *Complete Writings of Thomas Paine*, 1: 620. Few Americans followed Paine's logic in the eighteenth or nineteenth centuries.

amazing spirit, which has operated, and still will operate with great energy. Never before has the experiment been so effectually tried, of every man's reaping the fruits of his labor, and feeling his share in the aggregate system of power."¹⁰

The labor theory of property or value thus became the standard to explain a morally just distribution of wealth. When all laborers in a society received the full fruits of their labors, then the wealth distribution was a just and natural distribution. The nations of the world exhibited not equitable distributions of wealth but enormously lopsided ones. The revolutionaries sought to explain how natural wealth distributions became skewed. They did so by developing an understanding of the way aristocracies functioned economically.

THE SECOND PILLAR IN THE REVOLUTIONARY LEADERSHIP'S understanding of the distribution of wealth was the political economy of aristocracy, and it became the most important part of the theory in terms of assessing governmental policies. The phrase "the political economy of aristocracy" was not used by Americans in the eighteenth or nineteenth centuries; rather, it is a description I use for a set of attitudes, ideas, and policies that revolutionaries and their successors continually linked together.

In the last quarter of the eighteenth century, the obvious reference point for Americans was the political system of Europe and England, and the most visible feature of those nations was the presence of a social hierarchy, an aristocracy. For Americans, aristocracy became at least by the 1780s the natural enemy of republicanism, a perception that in the nineteenth century grew into a popular mania. Republicanism was founded on political equality; aristocracy was based on favoritism, hierarchy, and special privilege. Moreover, in its economic content, republicanism came increasingly to be identified with the natural right of the laborer to claim possession of the values his labor created. Aristocracy denied the labor theory of property or value in an explicit manner. An aristocrat, or more generally in England a Gentleman, was one who did not labor with his hands. David Ramsey in his history of the revolution (1789) made explicit the connection between labor and the lack of an aristocracy in America: "Every man occupied that rank only, which his own industry, or that of his near ancestors, had procured him." Thomas Jefferson revealingly commented to Thomas Cooper in 1814 that "our rich, who can live without labor, . . . [are] few, and of moderate wealth." Immediately after the revolution, Franklin warned would-be immigrants that aristocratic habits would not suffice in the New World: "In short, America is the land of labour."¹¹

¹⁰ Ezra Stiles, *The United States Elevated to Glory and Honor: A Sermon, Preached before his Excellency Jonathan Trumbull, Esq. L.L.D. . . . at the Anniversary Election May 8th, 1783* (New Haven, Conn., 1783), 35. See Edmund S. Morgan, *The Gentle Puritan: A Life of Ezra Stiles, 1727–1795* (New Haven, Conn., 1962), 213, 453.

¹¹ David Ramsey, *The History of the American Revolution*, 2 vols. (Philadelphia, 1798), 1: 31, 32; Thomas Jefferson to Thomas Cooper, September 10, 1814, in Andrew A. Lipscomb, ed., *The Writings of Thomas Jefferson*, library edn., 20 vols. (Washington, D.C., 1904), 14: 182; Benjamin Franklin, *Information to Those Who Would Remove to America* [March 1783], quoted in Carl Van Doren, *Benjamin Franklin* (New York, 1938), 704. See also Rev. James Madison to James Madison [Jr.], June 11, 1787,

For Americans traveling to Europe in the last half of the eighteenth century, the most salient aspect of European society was its horrid maldistribution of wealth—the few rich and the multitudinous poor. Many American wayfarers found the extent of poverty shocking.¹² Americans then connected the two aspects of European society and posited a causal relationship. Aristocracy, the social system of hierarchy and special advantages for the few, ruled the society of Europe; Europe had a maldistribution of wealth that favored the aristocracy. Therefore, the social system of aristocracy produced inequality and maldistribution of wealth.

The answer to how the social system of aristocracy generated wealth inequality was easily found. American political leaders applied the labor theory of property or value; an unjust distribution resulted when a few were able to transfer the fruits of other persons' labor to themselves. Aristocrats (non-workers and therefore non-producers) stole the fruits of labor from the masses of toilers (laborers and therefore producers). Aristocrats effected the transference by political means. It was control of politics that enabled aristocrats to steal the fruits of labor, to enrich themselves and pauperize the multitudes. By the time of the writing of the Constitution, literate Americans had clearly voiced the idea that a maldistribution of wealth was almost entirely a political act.¹³

During the colonists' struggle with Parliament over imperial regulation, American politicians identified the particular policies and institutions that aristocrats

in William T. Hutchinson, William M. E. Rachal, and David B. Madder, eds., *The Papers of James Madison*, 17 vols. (Chicago, 1962–), 10: 45; John Adams, "Twenty-Six Letters upon Interesting Subjects Respecting the Revolution of America" [letters written to Mc.Calkoen, of Amsterdam, 1780], letter of October 10, 1780, in Charles Francis Adams, ed., *The Works of John Adams, Second President of the United States . . .*, 10 vols. (Boston, 1856), 7: 279. On aristocracy in England, see Daniel A. Baugh, "Introduction," in Baugh, ed., *Aristocratic Government and Society in Eighteenth-Century England: The Foundations of Stability* (New York, 1975), 10. John Adams was incensed by the treatment the European nobility accorded him in the 1770s because they looked down on those who, in Edward Handler's phrase, "lived by the product of their own labor." Edward Handler, *America and Europe in the Political Thought of John Adams* (Cambridge, Mass., 1964), 85.

Americans wrangled over the question of a "natural aristocracy," but a natural aristocracy was always significantly different from the legal aristocracy in Europe. The natural aristocracy earned preeminence through talent, which in later times came to mean achievement, and achievement was revealed in some form of active labor. Although Gordon Wood has emphasized the monarchical quality of mid-eighteenth-century life, he has also stressed the anti-aristocratic thrust of the American Revolution that appeared by the 1780s; Wood, *Radicalism of the American Revolution*, 25–27, 36, 180–82, 237–43, 277. Isaac Kramnick has written that the essence of political movements in the Anglo-American world in the late eighteenth century was liberal, not republican, in the sense that industrious labor was glorified while aristocratic leisure was condemned; Isaac Kramnick, *Republicanism and Bourgeois Radicalism: Political Ideology in Late Eighteenth-Century England and America* (Ithaca, N.Y., 1990), 1–10.

¹² Thomas Jefferson to John Banister, Jr., October 15, 1785, in Julian P. Boyd and John Catanzariti, eds., *The Papers of Thomas Jefferson*, 24 vols. (Princeton, N.J., 1950–), 8: 637–38; Jonathan W. Austin, "Oration Delivered at Boston, March 5, 1778," in Niles, *Principles and Acts of the Revolution*, 53; Quincy, *Memoir of the Life of Josiah Quincy Jun.*, journal entries of November 8, 11, 13 [1774], pp. 224–26; John Adams to Elbridge Gerry, September 9 and December 12, 1783, in James T. Austin, *The Life of Elbridge Gerry*, 2 vols. (1828; New York, 1970), 1: 399–401; Dumas Malone, *Jefferson and His Time*, 6 vols. (Boston, 1948–81), 2: 108, 155; Richard B. Morris, *The American Revolution Reconsidered* (New York, 1967), 55; Richard K. Matthews, *The Radical Politics of Thomas Jefferson: A Revisionist View* (Lawrence, Kan., 1984), 31–32; William Cabell Bruce, *Benjamin Franklin Self-Revealed: A Biographical and Critical Study Based Mainly on His Own Writings*, 3d edn., 2 vols. (New York, 1942), 2: 171–76; Elizabeth P. McCaughey, *From Loyalist to Founding Father: The Political Odyssey of William Samuel Johnson* (New York, 1980), 71; Scott, *In Pursuit of Happiness*, 38–42.

¹³ See comment of Wood, *Radicalism of the American Revolution*, 5.

manipulated in order to create a skewed distribution of wealth. At this point, their ideas about the distribution of wealth mirrored the political theory of republicanism and were in fact derived from it. Republicanism in general taught that in any society there were men who grasped for power and dominion. These individuals led society out of virtue (willingness to sacrifice self-interest for the general welfare) into luxury and corruption. Part of the process of civil degeneration was involvement in wars, which established a large standing army and a huge national debt. The national debt necessitated high taxes to pay off the debt, and those high taxes fell upon the industrious part of the community. Political demagogues funneled tax money into an ever-growing army of political appointees and bureaucrats. With these governmental positions available—positions in which the labor was nonexistent but the reward great—despots could corrupt legislators and voters with offers of places and pensions and thereby obtain laws that elevated private, selfish interests above the public welfare.

Revolutionary leaders used republicanism in a political, not an economic, context; republicanism showed how a republic descended into political despotism. Yet an economic content dwelled in this analysis, an economic explanation that perhaps was not as apparent to the revolutionary generation as it would be to later ones. In their recourse to republicanism to find the policies and institutions that would perpetuate a republic, the leadership had also delineated the policies that aristocrats used to distort the distribution of wealth in their favor. Political republicanism had defined economic republicanism.¹⁴ In the contest with Parliament over imperial control of the colonies, American leaders warned that not only would specific policies bring about future political tyranny but such policies would also distort the American pattern of wealth distribution. And the key phrase indicating that various regulations portended an aristocratic transfer of wealth from the many to the few was an allusion to the labor theory of property or value, “the fruits of labor.”

According to the American leadership, the first aristocratic policy guilty of skewing the wealth distribution was taxation. Without giving the people a say in taxation policies (that is, representation), governments—especially aristocratic ones—could tax away the proceeds of honest labor. Thus Alexander Hamilton warned that Parliament’s attempt to tax the colonists without their consent meant that the colonist’s “fruits of his daily toil are consumed in oppressive taxes.” Elbridge Gerry wrote in 1774 that the “point [of the conflict] is, whether Americans shall enjoy the fruits of their labour, or send them in taxes to Great Britain.”¹⁵

¹⁴ This summary of republicanism draws largely on Pocock, *Machiavellian Moment*, 424–61, 468–504; Banning, *Jeffersonian Persuasion*, 46–48, 67–70, 75–77; McCoy, *Evasive Republic*, 5–10, 16–34, 62, 67–73; James M. Banner, Jr., *To the Hartford Convention: The Federalists and the Origins of Party Politics in Massachusetts, 1789–1815* (New York, 1970), 26–27, 39, 49.

¹⁵ Alexander Hamilton, *A Full Vindication of the Measures of Congress, &c* [1774], in Harold C. Syrett and Jacob E. Cooke, eds., *The Papers of Alexander Hamilton*, 27 vols. (New York, 1961–87), 1: 53; Elbridge Gerry to Samuel Adams, May 12, 1774, in Austin, *Life of Elbridge Gerry*, 1: 44. See also Sam Adams, “The Rights of the Colonists, A List of Violations of Rights and a Letter of Correspondence,” November 20, 1772, in Cushing, *Writings of Samuel Adams*, 2: 360; speech of John Rutledge, April 11, 1776, in Niles, *Principles and Acts of the Revolution*, 326; Richard L. Bushman, “Massachusetts Farmers

The second wealth-distorting policy of aristocratic governments was the establishment of a monstrous bureaucracy filled with appointees, pensioners, the military, and churchmen. These bureaucrats performed no productive labor. In endless speeches and tracts, revolutionaries insisted that the use of tax monies to pay bureaucrats was equivalent to stealing the fruits of labor from industrious producers and awarding those fruits to non-producers. Alexander Hamilton made the transfer of wealth explicit: "How would you like to pay four shillings a year, out of every pound your farms are worth, to be squandered, (at least a great part of it) upon ministerial tools and court sycophants? What would you think of giving a tenth part of the yearly products of your lands to the clergy?" Sam Adams constantly warned that the sole purpose of British taxation was "to feed and pamper a set of infamous wretches, who swarm like the locusts of Egypt; and some of them expect to revel in wealth and riot on the spoils of our country." The townspeople of Bristol, Rhode Island, complained in a set of resolutions in 1774 that it was intolerable "that so many unnecessary officers are supported by earnings of honest industry, in a life of dissipation and ease; who, by being *properly* employed, might be useful members of society."¹⁶

The third policy by which an aristocratic government could threaten a natural distribution of wealth was government favoritism to certain people or companies, and the example in the revolutionary period was Britain's bestowing upon the East India Company a monopoly to supply tea to North America. American leaders made the economic connection easily; favoritism meant economic monopoly, and monopoly enriched the few at the expense of the many. John Dickinson exemplified how the fruits of labor rhetoric was used in this instance. He raised the question of whether "WE, our WIVES and CHILDREN, together, with the HARD EARNED FRUITS OF OUR LABOUR, are not *made over to this* almost *bankrupt Company*, to augment their Stock, and to *repair* their ruined Fortunes."¹⁷

The fourth policy guilty of skewing an equitable wealth distribution was the erection of an established church, and the clear example was the Church of England. Americans feared British ecclesiastical interference in their religious domains, of course, but they were also very much aware of what the "tithe" had done to the English people. Moreover, many Americans, like John Adams, saw an established church as another means by which the British government could

and the Revolution," in Richard M. Jellison, ed., *Society, Freedom, and Conscience: The American Revolution in Virginia, Massachusetts, and New York* (New York, 1976), 123.

¹⁶ Hamilton, *Full Vindication*, in Syrett and Cooke, *Papers of Alexander Hamilton*, 1: 67; Candidus [Samuel Adams], *Boston Gazette*, October 14, 1771, in Cushing, *Writings of Samuel Adams*, 2: 253; Bristol Resolves quoted in Morgan, *Challenge of the American Revolution*, 105. See James Haw, Francis F. Beirne, Rosamond R. Beirne, and R. Samuel Jett, *Stormy Patriot: The Life of Samuel Chase* (Baltimore, Md., 1980), 24; Morgan, *Challenge of the American Revolution*, 103; Bushman, "Massachusetts Farmers and the Revolution," 82–85, 123–24.

¹⁷ John Dickinson, *Two Letters on the Tea Tax* [1773], in Ford, *Political Writings of John Dickinson*, 459. For an example of an American who understood the economic ramifications of monopoly, see George Mason to Richard Henry Lee, May 18, 1776, in Robert A. Rutland, ed., *The Papers of George Mason, 1725–1792*, 3 vols. (Chapel Hill, N.C., 1970), 1: 272. The attack on monopolies had something of a radical Protestant quality to it because English Protestant radicals had attacked monopolies for nearly 150 years before the American Revolution; see G. P. Gooch, *English Democratic Ideas in the 17th Century* (1898; New York, 1959), 33, 58, 128–29, 171–72; Lacey Baldwin Smith, *This Realm of England, 1399 to 1688* (Lexington, Mass., 1966), 229–35.

extend its bloated bureaucracy and award positions to governmental supporters.¹⁸

The fifth policy that political leaders believed warped the distribution of wealth out of its natural state did not come from protest over British policies in the 1760s and 1770s but from American conditions in the 1780s. The issue was the appropriate medium of exchange: specie or paper money. Although the paper money controversy came at the end of the revolutionary era (broadly defined as 1763 to 1789), the subject easily became incorporated into the constellation of policies comprising the political economy of aristocracy. The choice of a standard of value was a governmental decision. The connection between government and a standard of value evoked the fear of aristocratic manipulation: government could be used to transfer the proceeds of labor from the many to the few. It was for this reason that currency and banking issues were so readily absorbed into the political economy of aristocracy.

Yet the currency situation in the postwar United States had unusual complications. The derangements of the war and the ensuing depression left Americans with a large national debt, spiraling prices, and a lack of specie. To circumvent the problems of an insufficient money supply, state governments (as well as the Continental Congress) tried at various times to regulate prices and to emit a paper money. Economic difficulties led to intense debates between creditors and debtors, often bringing into question the operations of a marketplace during a crisis. Populist debtor groups feared merchants' potential for monopoly profits and upheld an older tradition of popular and governmental intervention in the economy to procure the common good. The conservative and propertied creditor class had been absorbing the market dynamics of the Atlantic economy and insisted on allowing prices to reflect the forces of supply and demand—in effect, they argued for capitalist economics, the doctrines of which had only recently been synthesized by Adam Smith.¹⁹

¹⁸ Richard Henry Lee to William Lee, June 19, 1771, in James Curtis Ballagh, ed., *The Letters of Richard Henry Lee*, 2 vols. (1911; New York, 1970), 1: 59; John Adams to Dr. Morse, December 2, 1815, in C. F. Adams, *Works of John Adams*, 10: 185. See also J. Adams, "Twenty-Six Letters upon Interesting Subjects," 7: 279; Noah Webster, Jr., *Effects of Slavery on Morals and Industry* (Hartford, Conn., 1793), 31; Ramsey, *History of the American Revolution*, 1: 31; J. Hector St. John de Crèvecoeur [Michel-Guillaume-Jean de Crèvecoeur], *Letters from an American Farmer* [1782] (New York, 1912), 44, 87; Dickinson, *Letters of a Farmer in Pennsylvania*, 374–75; House of Representatives of Massachusetts to Denny De Berdt, January 12, 1768, in Cushing, *Writings of Samuel Adams*, 1: 149; Roger Sherman Boardman, *Roger Sherman: Signer and Statesman* (1938; New York, 1971), 103–04; H. James Henderson, "Taxation and Political Culture: Massachusetts and Virginia, 1760–1800," *William and Mary Quarterly*, 3d ser., 47 (January 1990): 92, 103–05. Several states, most obviously Connecticut and Massachusetts, did not separate church and state until the second decade of the nineteenth century. The thrust of the revolutionary analysis of the distribution of wealth was toward separation; in some cases, however, it took a number of years to be achieved.

¹⁹ For example, see Ronald Schultz, "Small Producer Thought in Early America, Part I: Philadelphia Artisans and Price Control," *Pennsylvania History*, 54 (April 1987): 115–47, esp. 121, 135–36; A Committee of Congress to the Several States, November 11, 1778, in Edmund C. Burnett, ed., *Letters of Members of the Continental Congress*, 8 vols. (Washington, D.C., 1921–36), 3: 491; Richard B. Morris, "Labor and Mercantilism in the Revolutionary Era," in Morris, ed., *The Era of the American Revolution: Studies Inscribed to Everts Boutell Greene* (New York, 1939), 88–89, 120–23; Jackson Turner Main, *The Sovereign States, 1775–1783* (New York, 1973), 235–56; Edward Countryman, *A People in Revolution: The American Revolution and Political Society in New York, 1760–1790* (Baltimore, Md., 1981), 186.

The wrangling over the issue of paper money evoked the key features in the American concept of the distribution of wealth and revealed as well the points of social conflict that it failed to resolve. The populist portion of the nation favored the emissions, but their advocates lacked economic principles on which to base their claims; leaders of paper money forces frankly admitted that they espoused paper money because of the exigencies of the moment—they relied ultimately on the idea of expediency.²⁰ They also justified their actions with a reading of the labor theory of property or value. In the Massachusetts insurrection of 1786–1787, small farmers saw their farms being lost because of forces unleashed by the war and its aftermath, for which they had no personal responsibility. Those farms were the products of labor and toil, the fruits of their labors. Paper money advocates argued that the issuance of paper money broke some rules, was expedient, but also preserved the fruits of labor of the masses.²¹

The conservative, hard-money side won the argument for essentially one reason: conservatives tied currency to the principle that labor only received its just reward (fruits) when the standard of value remained constant and was not subject to manipulation. Their presentation offered an eternal principle, not expediency; and, to a culture that tended to see human society as an analog to Newtonian celestial mechanics, eternal laws obtained a wider respect than did a frank avowal of contingency. Ironically, the dynamics of their reasoning, especially their insistence that the laborer obtain exactly the value his labor produced, led to an inverse of positions on paper currency in the nineteenth century. Then the popular side of the controversy—indeed, the working-class side—demanded a specie-based currency, while the expansionist elite desired a form of paper money.

Specie advocates reasoned that whatever served as the medium of exchange had to possess a stable, non-fluctuating value. In market activities, exchange meant exchanging the fruits of one person's labor for the fruits of another. If the value of the medium of exchange varied, then someone obtained more (or less) than he deserved. Effectively, the financial situation cheated someone out of the "fruits of his labor." Specie maintained a fairly constant value, but paper money did not. Thus specie was the superior and in fact the morally just medium of exchange. George Mason helped write resolutions in Virginia in 1787 that called for a specie currency: "[N]othing can be more improper and unjust than to substitute such a standard [of value] as wou'd be more uncertain and variable than the Commodities themselves." John Adams succinctly explained to James Warren in 1777 the distributive imperatives governing currency: "[T]he medium of trade ought to be as unchangeable as truth, as immutable as morality. The least variation in its value does injustice to multitudes, and in proportion it injures the morals of the people, a point of the last importance in a republican government."

²⁰ On the paper money dispute, see Jackson Turner Main, *The Antifederalists: Critics of the Constitution, 1781–1788* (New York, 1961), 7–8, 26–27, 31–36, 49–50, 46–50, 53–71; Main, "The American States in the Revolutionary Era," in Ronald Hoffman and Peter J. Albert, eds., *Sovereign States in an Age of Uncertainty* (Charlottesville, Va., 1981), 9–16; Clifton B. Luttrell, "Thomas Jefferson on Money and Banking: Disciple of David Hume and Forerunner of Some Modern Monetary Views," *History of Political Economy*, 7 (Summer 1975): 157–65.

²¹ See David Szatmary, *Shays' Rebellion: The Making of an Agrarian Insurrection* (Amherst, Mass., 1980), 1–6, 32–34, 40, 45–47, and *passim*; Ramsey, *History of the American Revolution*, 2: 134–35.

Tom Paine also deprecated the substitution of paper notes for specie, and he tied gold and silver directly to the labor theory of property or value: "Money, when considered as the fruit of many years' industry, as the reward of labor, sweat and toil, as the widow's dowry and children's portion, . . . has something in it sacred that is not to be sported with."²²

These five policies—high taxation, extensive bureaucracy, financial and currency manipulations, special privileges and monopolies, and established churches—represented the concrete actions aristocratic governments took to warp the distribution of wealth in favor of the few and to the detriment of the many. The political economy of aristocracy posited that in lands characterized by political monopoly, rulers—monarchs or aristocrats—used governmental policies to enrich themselves and their retainers, thus producing a maldistribution of wealth. By possessing the military power of the state, the rulers ensured that their policies were obeyed by the populace. Those who monopolized government taxed away the fruits of labor of the people, used the funds to enrich themselves and create offices for fawning dependents, obtained monopolies for themselves, manipulated finances for self-aggrandizement, and used an established church to support the sons of the nobility and court favorites. The action of these policies over time created the few rich and the many poor.

The revolutionary leadership's diagnosis of the political economy of aristocracy had a number of important features and consequences. The Americans engaged in what today would be called a "systems analysis." The inordinate treasures of a few and the unremitting poverty of the many did not come from nature but from a social system; a social system led to wealth extremes. Americans found the source of economic ills in the system of aristocracy. It was understandable that Americans should think of aristocracy as the problem, for it was the preeminent social edifice of Europe, where the maldistribution of wealth was so extensive. The analysis accorded with observable fact and was logical in its construction, but literate and articulate Americans in the eighteenth and nineteenth centuries never quite confronted the possibility that they had chosen the wrong social system to explain wealth and poverty.²³

The American understanding of the political economy of aristocracy led inescapably to the policies a republic should establish. If aristocracies created inequalities by high taxes, a large bureaucracy, legislatively sanctioned monopolies, an established church, and currency manipulations, then a republic could politically create equality by implementing the opposite of aristocratic policies: low taxes, no bureaucracy, disestablishment of church and state, abolition of monopolies, and a stable medium of exchange. Since the principles of aristocracy were the opposite of republicanism, so, the reasoning went, the policies of republicanism should be the opposite of aristocracy. What was happening was the embrace of laissez-faire principles by American republicans on the premise that

²² Resolution of Virginia legislature in Helen Hill, *George Mason: Constitutionalist* (Cambridge, Mass., 1938), 176; John Adams to James Warren, February 12, 1777, in C. F. Adams, *Works of John Adams*, 9: 455; Thomas Paine, *Dissertation on Government*, in Foner, *Complete Writings of Thomas Paine*, 2: 404–09, quote on 405.

²³ Crèvecoeur is a good example of how one person explained poverty in terms of the existence of aristocracy; Crèvecoeur, *Letters from an American Farmer*, 39–43, 44, 83–87, 91–92.

laissez-faire policies would produce an equitable distribution of wealth because the policies were the opposite of those established by aristocratic governments.²⁴

THE THIRD AXIOM OF THE AMERICAN UNDERSTANDING OF the distribution of wealth was the role of the laws of primogeniture and entail in producing extreme concentrations of wealth in England. Literate Americans living in the late eighteenth century entertained no doubts that primogeniture and entail were among the most important props of aristocratic society and generators of inequality. As with governmental policies generally, Americans reasoned that by removing the props of aristocracy and inequality, they could achieve republicanism and equality. The English radical Richard Price lectured Americans on the actions they had to take to preserve the fruits of the revolution, and he was emphatic on the necessity of abolishing the law of primogeniture, as the "tendency of this [law] to produce an improper inequality is very obvious." When in France, Franklin wrote that Europe had two attitudes that hindered development: "one, that useful labor is dishonorable; the other, that families may be perpetuated with estates." Many Americans looked favorably on the inheritance laws of New England, which required partible inheritance when possessors of wealth died intestate. John Adams in 1778 figured that no real concentrations of wealth in New England would ever occur, and a fundamental equality of property would always exist because of partible inheritance: "The tendency of the laws of inheritance [in New England] is perpetually to distribute and subdivide whatever portion of land acquires any great market value." Although historians have generally been unimpressed with the abolition of the laws of primogeniture and entail in the new republic, Americans of that time saw it as one of the revolution's greatest achievements and guarantors of republican equality.²⁵

²⁴ It is no coincidence that American republicanism came to embrace the policies of laissez faire being promulgated by the first classical economist, Adam Smith. The *Wealth of Nations* by Smith mirrors many of the same complaints of the American revolutionaries, even though Smith did not declare himself a republican. The tie that bound Adam Smith and the American revolutionaries together was an attack on the principles and institutions of aristocracy, Smith from an economic direction and the Americans from a political direction. Although Smith's work was formally an attack on the theory of mercantilism, it was in a broader sense a critique of the economic results of aristocracy. On the subject of governmental policies, the Americans and Smith converged and produced the same set of anti-aristocratic recommendations. See Adam Smith, *An Inquiry into the Nature and Causes of the Wealth of Nations*, Edwin Cannan, ed. (1776; New York, 1937), 30–33, 49, 60, 64, 118–21, 145, 314–15, 423–29, 433–35, 716; and commentary of William D. Grampp, "Adam Smith and the American Revolutionists," *History of Political Economy*, 11 (Summer 1979): 179–91; Donald Winch, *Adam Smith's Politics*, 7–8, 43, 81–84, 87–90, 148, 181; James T. Kloppenberg, "The Virtues of Liberalism: Christianity, Republicanism, and Ethics in Early American Political Discourse," *Journal of American History*, 74 (June 1987): 17, 18; Jacob Viner, *The Long View and the Short: Studies in Economic Theory and Policy* (Glencoe, Ill., 1958), 218, 225, 231, 234, 240; Glenn R. Morrow, *The Ethical and Economic Theories of Adam Smith* (1923; New York, 1969), 64–65, 76; Ralph Lerner, "Commerce and Character: The Anglo-American as New-Model Man," *William and Mary Quarterly*, 3d ser., 36 (January 1984): 6; Kramnick, *Republicanism and Bourgeois Radicalism*, 8–10.

²⁵ Richard Price, "Observations on the Importance of the American Revolution, and the Means of Making It a Benefit to the World . . .," in Bernard Peach, ed., *Richard Price and the Ethical Foundations of the American Revolution* (Durham, N.C., 1970), 209; Benjamin Franklin to the count de Cam-pomanes, June 5, 1784, in John Bigelow, ed., *The Life of Benjamin Franklin, Written by Himself*, 3d edn., 3 vols. (Philadelphia, 1893), 3: 268; John Adams, *A Defence of the Constitutions of the United States of America, Against the Attack of M. Turgot, in his letter to Dr. Price, dated the Twenty-Second day of March*,

The fourth axiom that made up the American concept of the distribution of wealth was the weakest. It was also non-political and broke with the tendency to find the source of inequality in politics. The fourth axiom was the population-to-land ratio. Awareness of population pressure had evidently been sinking into the European consciousness for some time, and it was picked up by astute Americans who recognized that their land had far fewer people than did the nations of Europe.

Two individuals most cognizant of the distortions in society and equality that population pressure could generate were James Madison and Thomas Jefferson, with Madison being the one most impressed with the deleterious potential of explosive population growth. Although Madison acknowledged the evils aristocratic politics could inflict on a society, he believed that the political economy of aristocracy could not explain all the misery in the world. In June 1786, Madison and Jefferson wrote to each other discussing the sources of poverty. Madison had no doubt "that the misery of the lower classes will be found to abate wherever the Government assumes a freer aspect, and the laws favor a subdivision of property," by which statement he concurred with the current view that many aristocratic social ills could be cured by application of republican remedies. Yet he saw a limitation to what those republican remedies could accomplish: "A certain degree of misery seems inseparable from a high degree of populousness."²⁶

By the time of the adoption of the Constitution, the belief that the malevolence of the system of aristocracy created an unequal distribution of wealth had become widespread among American political leaders and probably among much of the public as well. Among the literate, the forces that shaped and distorted a nation's distribution of wealth were adequately encapsulated in the four axioms that comprised the American concept of the distribution of wealth. Those four axioms were not the only notions voiced by Americans in the revolutionary era to explain the distribution of wealth, but they were the prominent ones.²⁷

1778, in C. F. Adams, *Works of John Adams*, 4: 359–60. Adams later became more pessimistic and believed families would dominate politics and accumulate wealth regardless of the land laws; John Adams to John Taylor, April 15, 1814, in *ibid.*, 6: 459–61, 507–08; John Adams to Thomas Jefferson, July 16, 1814, in Lipscomb, *Writings of Thomas Jefferson*, 14: 157–58. See also Thomas Jefferson, "Autobiography," in Lipscomb, *Writings of Thomas Jefferson*, 1: 64.

For the secondary literature about the abolition of primogeniture and entail in the states—most containing doubts as to the importance of the abolition—see Carole Shammas, Marylynn Salmon, and Michel Dahlin, *Inheritance in America from Colonial Times to the Present* (New Brunswick, N.J., 1987), 30–35, 56, 65–67; Stanley N. Katz, "Republicanism and the Law of Inheritance in the American Revolutionary Era," *Michigan Law Review*, 76 (November 1977): 5, 11–20, 26–29; Morris, *American Revolution Reconsidered*, 79–81; J. Franklin Jameson, *The American Revolution Considered as a Social Movement* (1926; Princeton, N.J., 1967), 36–39; Robert A. Nisbet, *The Social Impact of the Revolution* (Washington, D.C., 1973), 8–12; Robert E. Brown and Katherine Brown, *Virginia, 1705–1786: Democracy or Aristocracy?* (East Lansing, Mich., 1964), 91–92, 286–87; Toby L. Ditz, *Property and Kinship: Inheritance in Early Connecticut, 1750–1820* (Princeton, 1986), 24–29, 47–52, 61–67, and *passim*. One of the few historians (cliometricians) to stress the economic importance of the abolition of entail and primogeniture on the American wealth distribution is Lee Soltow, *Distribution of Wealth and Income in the United States in 1798* (Pittsburgh, Pa., 1989), 111, 126–35, 144.

²⁶ James Madison to Thomas Jefferson, June 19, 1786, in Boyd and Catanzariti, *Papers of Thomas Jefferson*, 9: 659–60; McCoy, *Elusive Republic*, 126–27, 189–203.

²⁷ Americans sometimes found any rich person guilty of creating poverty. Many blamed poverty on the character traits of the poor; great numbers denounced individuals who practiced engrossment, monopoly, and sharp dealing. Some well-read individuals, like John Adams, feared that commerce inevitably produced unequal distributions of property. Examples of these other ways of

But even by 1790, American ideas about wealth distribution had disturbing limitations.

The American concept of the distribution of wealth originated with the revolutionary leadership and therefore was a product of the colonial upper class; its elaboration was a written and verbal articulation that of itself bespoke upper-class origins. Nonetheless, the revolutionaries' ideas about wealth distribution seem to have penetrated deeply into the layers of American society. Two axioms of the theory remained abstract and generally the concern of the literate: the laws of entail and primogeniture and the population-to-land ratio. But the two postulates of the labor theory of property or value and the political economy of aristocracy gained a foothold throughout the social structure. In the rudimentary state of late colonial society, when the act of working for one's income was prevalent, the idea that the laborer deserved "the fruits of his labor" could appeal to individuals in every social stratum. Moreover, given that mid-eighteenth-century Americans only obtained the status of colonials within the British empire, it requires little imagination to assume that Americans were worried about the designs that aristocrats in the imperial center might have on the fruits of American prosperity. As a colonial people, it can be assumed, Americans were sensitive to the possibility of being legally manipulated to advance the interests of a select few three thousand miles away. A fear of being manipulated could be shared by all classes in American society; the trepidation thus served as the basis for a broad acceptance of the warnings contained in the political economy of aristocracy.

Divisions over the practical application of the leadership's depiction of the forces controlling the distribution of wealth were unavoidable from the start. Generalities might be agreed on, but specifics could reveal class tensions. The difficulty was the literary nature of the discourse. No one supplied any numbers or any standards of evaluation. What was considered an acceptable distribution of wealth to a member of the upper class might not appear to be at all just to a member of the lower class—and in the nineteenth century, this point became the locus of contention. Similarly, while virtually all Americans said the laborer deserved "the fruits of his labor," no one proposed a means to determine quantitatively what those fruits were. Over time, the upper class settled on market forces for a quantitative determination, a position usually eliciting reservations from those on the lower end of the income scale.

In one particular area, the failure to provide precise descriptions revealed the path for class divergence over the republican theory of the distribution of wealth.

explaining wealth distribution can be found in [John F. Mercer?], "Essays by a Farmer," No. 2, in Herbert J. Storing, ed., *The Complete Anti-Federalist*, 7 vols. (Chicago, 1981), 5: 18–20; Dirk Hoerder, *Crowd Action in Revolutionary Massachusetts, 1765–1780* (New York, 1977), 76–77; Dr. John Warren, *An Oration Delivered July 4, 1783, at the Request of the Inhabitants of the Town of Boston, in Celebration of the Anniversary of American Independence* (Boston, 1783), 11–13; Marquis de Chastellux, *Travels in North America in the Years 1780, 1781, and 1782*, revised trans. by Howard C. Rice, Jr., 2 vols. (Chapel Hill, N.C., 1963), 1: 161; Gary B. Nash, *Red, White, and Black: The Peoples of Early America*, 2d edn. (Englewood Cliffs, N.J., 1982), 265–66; Spartacus, "The Interest of America" [June 1776], in Richard B. Morris, ed., *The American Revolution, 1763–1783: A Bicentennial Collection* (Columbia, S.C., 1970), 295–96; McCoy, *Elusive Republic*, 17–24, 36–40.

At the time of the revolution, the leadership insisted that the fruits of labor included mental as well as manual labor. Since the society depended so largely on manual labor, little dissonance surfaced over this point. When the American economy metamorphosed into a larger market economy in the middle of the nineteenth century, the income reward between mental and manual labor began to widen. Protests over the leadership's extensive definition of the "fruits of labor" began to rise accordingly.²⁸

Even within their own framework, the leadership's handling of the question of describing an equitable distribution of wealth was too broad. They pointedly argued that inequalities were bound to arise because talents were not equally distributed. They never revealed how much inequality a "good" distribution of wealth permitted. Evidently, the revolutionary leadership envisioned a widespread diffusion of property more than an equal distribution of it. Empirical studies of the colonial distribution of wealth have found disturbingly large imbalances. Because American leaders easily excused their own wealth inequalities, it seems fair to conclude that they were more concerned with an absolute than a relative distribution of property. For a republic, widespread ownership of property, even in small lots with a few large accumulations, was permissible. Property ownership, regardless of size, provided independence, not relative amounts of it. Size of property holdings, for the political leaders, only became important in extremes when the impoverished class became too great and the wealthy class too small.²⁹

In another area, the revolutionary leadership's ideas about the distribution of wealth not only exhibited weakness and ambiguity but bordered on hypocrisy. The American discussion about the importance of the distribution of wealth to the permanency of a republican form of government implies that the leaders would favor rules enforcing a sufficient equality of property to guarantee the republic's survival. But, except for the abolition of the laws of primogeniture and entail, the revolutionary leadership never favored laws that forced a redistribution of property (agrarian laws), even though republican theorists such as Montesquieu, Harrington, Francis Hutcheson, and Mrs. Macaulay had partially approved of such laws.³⁰

One obtains the impression that important politicians, upon discovering that their constitutional struggle with Great Britain was serious and leading them to

²⁸ For the middle decades of the nineteenth century, see Jonathan A. Glickstein, *Concepts of Free Labor in Antebellum America* (New Haven, Conn., 1991), 23–52, and *passim*.

²⁹ It need hardly be stated that investigations into the distribution of wealth in colonial and revolutionary times have established that wealth was very unequally distributed, the top 10 percent of the population possessing roughly 50 percent of colonial net worth. See Alice Hanson Jones, *Wealth of a Nation to Be: The American Colonies on the Eve of the Revolution* (New York, 1980), table 6.1, pp. 162–63; Jeffrey G. Williamson and Peter H. Lindert, *American Inequality: A Macroeconomic History* (New York, 1980), 9, 28–30, 36–39; Soltow, *Distribution of Wealth and Income in the United States in 1798*, 41–48, 81–82; Carole Shammas, "A New Look at Long-Term Trends in Wealth Inequality in the United States," *AHR*, 98 (April 1993): 416–24.

³⁰ Caroline Robbins, *The Eighteenth-Century Commonwealthman: Studies in the Transmission, Development and Circumstance of English Liberal Thought from the Restoration of Charles II until the War with the Thirteen Colonies* (Cambridge, Mass., 1959), 15, 125, 191–92; Scott, *In Pursuit of Happiness*, 27; Pocock, *Machiavellian Moment*, 387, 468; Thomas L. Pangle, *Montesquieu's Philosophy of Liberalism: A Commentary on "The Spirit of the Laws"* (Chicago, 1973), 77, 149–50.

form a republic, knew from their reading of past philosophers that a wholesome distribution of wealth was vital for a republic's health. They then with a sigh of relief, and without any numerical investigation into the matter, determined that the American distribution of wealth as it stood in 1776 was sufficient for a republic. Upon further reflection, they outlined a series of policies that should be avoided if they were not to create monstrous divergences in the distribution of wealth, but they also realized that, over time, such divergences would inevitably come about via population pressures. They were thankful that circumstance, the frontier, and simple luck had given them a healthy distribution of wealth. And, realizing that they would not have to go through the anguish and struggle of recasting institutions, they were mostly thankful that they did not have to do anything at all.

DESPITE ITS LIMITATIONS, THE REVOLUTIONARY ANALYSIS of the distribution of wealth survived through the whole of the nineteenth century unscathed, unaltered, and unqualified. Unlike the political theory of republicanism, which began mutating as soon as the revolutionary war ended, the American republican analysis of the distribution of wealth became even more precise and refined and was used on every economic issue that reached the national stage.³¹ Throughout the nineteenth century, Americans relied on the four axioms of the revolutionary leadership's theory of wealth distribution—the labor theory of property or value, the political economy of aristocracy, the laws of entail and primogeniture, and the population-to-land ratio—to explain undesirable wealth variations. The revolutionary leadership's theory of wealth distribution was often invoked in the nineteenth century, but space limitations demand that only the following suggestive examples be given to show how it was employed.

Scholars have somehow missed the persistent American discourse over the impact of the laws of entail and primogeniture on wealth distribution. Tariff debates often elicited comments about the importance of entail and primogeniture in creating aristocratic wealth imbalances, and in Henry Clay's famous March 1824 speech on the tariff, he pointed to the American "law of distributions" (partible inheritance) as the true means of maintaining wealth diffusion in the United States. In the first half of the nineteenth century, Americans also continually wrote about the social cataclysm that was Great Britain, and those commentaries always singled out the aristocratic laws of entail and primogeniture as important factors in the creation of a pauperized working class. Finally, this feature of American society captured the imagination of foreign visitors. They had no doubt that the granite foundation of American equality was partible

³¹ Original republicanism was based on property ownership, an avoidance of party organizations, virtue, agrarian occupations, and abhorrence of patronage systems. The Constitution recognized by 1789 that self-interest informed politics, a party system emerged in the 1790s, property qualifications for voting and holding office started to be eliminated in the 1820s, Andrew Jackson's presidency led to an acknowledged use of patronage as a glue for party organizations (the spoils system), and sometime between 1790 and 1880, depending on the source, public virtue mutated into private virtue and thence into economic individualism. Exactly when republicanism died has become a large academic industry in its own right.

inheritance and the abolition of entail and primogeniture. Francis J. Grund, German immigrant and soon-to-be vociferous Jacksonian Democrat, summarized the attitude of many European observers: the absence of primogeniture had "done more towards equalizing conditions than the spirit of exclusiveness will ever be able to overcome."³²

The influence of the population-to-land ratio on the distribution of wealth, the feeblest of the four axioms of the American concept of distribution in the eighteenth century, became stronger and more impressive in American thought as the nineteenth century wore on. Even Henry Clay argued that an available West (ignoring, as always, the presence of Native Americans) guaranteed high wages and thus an equitable wealth distribution. For agrarians and free trade advocates, the population-to-land ratio became the most important fact in the American distribution of wealth, a conclusion agreed to by many foreign visitors. The population-to-land ratio swamped the debates over western land disposal and homestead proposals. The possibility of preserving a European agrarian culture in North America became inseparably tangled with the population-to-land ratio and was the basis of "manifest destiny." And the economic analysis behind the population-to-land ratio, the idea that wages necessarily fell to a minimum level sufficient only to enable workers to survive physically, found a resounding welcome among proslavery writers. Proslavery tracts simply oozed Malthusianism.³³

In the nineteenth century, Americans elevated the labor theory of property or value to holy economic writ. This theory swamped American writing and speaking on every subject imaginable. Visiting Europeans identified the American belief in the nobility of labor as being the economic antagonist of aristocratic

³² Henry Clay, speech of March 31, 1824, *Annals of Congress*, 18th Cong., 1st sess., p. 1993; Francis J. Grund, *The Americans in Their Moral, Social and Political Relations* (1837; New York, 1968), 22. See Daniel Webster, "First Settlement of New England" [1820], in J. W. McIntyre, ed., *The Writings and Speeches of Daniel Webster*, 18 vols. (Boston, 1903), 1: 211-14; Horace Greeley, "The Protection of Industry," *Whig Almanac* for 1843, 11; Calvin Colton, *Public Economy for the United States* (New York, 1848), 154-56, 161, 163; "The Distribution of Property," *North American Review*, 67 (July 1848): 126-34, 137-52; "Distribution of Wealth," *Southern Review*, 8 (November 1831): 173, 179-90; "Macaulay's *History of England*," *Massachusetts Quarterly Review*, 2 (June 1849): 345-46; G. T., "Primogeniture and Entail," *United States Magazine and Democratic Review*, 25 (July 1849): 17-23; Frederick Douglass to William Lloyd Garrison, May 23, 1846, in Philip S. Foner, ed., *The Life and Writings of Frederick Douglass*, 4 vols. (New York, 1950), 1: 167; Alexis de Tocqueville, *Democracy in America*, Francis Bowen and Phillips Bradley, eds., 2 vols. (1834, 1840; New York, 1945), 1: 46-51, 293; Thomas Hamilton, *Men and Manners in America*, 2d edn. (1843; New York, 1968), 61-62; Adam G. de Gurowski, *America and Europe* (New York, 1857), 118; Alexander MacKay, *The Western World; or, Travels in the United States in 1846-47 . . .*, 2d edn., 3 vols. (1849; New York, 1968), 3: 341-42; Isaac Holmes, *An Account of the United States of America, Derived from Actual Observation during a Residence of Four Years in That Republic* (1823; New York, 1974), 102; Michael Chevalier, *Society, Manners, and Politics in the United States*, John William Ward, ed. (1839; Garden City, N.Y., 1961), 396-97; Basil Hall, *Travels in North America in the Years 1827 and 1828*, 3 vols. (1829; New York, 1974), 2: 320-31; Francis Pulszky and Theresa Pulszky, *White Red Black: Sketches of Society in the United States during the Visit of Their Guest*, 3 vols. (1853; New York, 1968), 3: 172.

³³ Clay, speech of March 31, 1824, *Annals of Congress*, 18th Cong., 1st sess., pp. 1986, 1991, 1994; Friends of Domestic Industry, *Address of the Friends of Domestic Industry, Assembled in Convention, at New-York, October 26, 1831, to the People of the United States* (Baltimore, Md., 1831), 20, 21; James L. Huston, *The Panic of 1857 and the Coming of the Civil War* (Baton Rouge, La., 1987), 99-102; Thomas Hietala, *Manifest Design: Anxious Aggrandizement in Late Jacksonian America* (Ithaca, N.Y., 1985), 96-101, 105-17; Paul Bergeron, *The Presidency of James K. Polk* (Lawrence, Kan., 1987), 66-68; James C. Hite and Ellen J. Hall, "The Reactionary Evolution of Economic Thought in Antebellum Virginia," *Virginia Magazine of History and Biography*, 80 (1972): 476-88.

leisure, passivity, and foppery. Labor was presented in the United States, wrote Alexis de Tocqueville, "on every side, as the necessary, natural, and honest condition of human existence." Two Hungarian visitors who ventured into the United States with Louis Kossuth offered a glowing picture of democratic practices: "yet the wonderful effect of democratic institutions strikes me always afresh. The principle, that *labour is never degrading*, is here carried into life." Michael Chevalier noted about the American republic that "this is the first time since the origin of society that the people have fairly enjoyed the fruits of their labor and have shown themselves worthy of the prerogatives of manhood. Glorious result!"³⁴

Of course, the American concept of the distribution of wealth was a verbal and intellectual abstraction that primarily acted as a guide for the formulation of economic policy. The heritage of colonial governance included a significant amount of intervention in economic affairs; and, in the first half of the nineteenth century, state governments used their powers to prod their states into full market economies by establishing chartered banks, transportation companies, and other monopolistic enterprises. Legislators exercised pragmatism, not ideology, in devising economic legislation; to foster economic growth—which most state legislators seemed desirous of doing—concessions such as monopoly privileges to investors (usually under some public control) seemed the only practical means of accomplishing certain specific tasks. Throughout the years of the most active state-government involvement (roughly 1790 to 1850), however, the attack on state intervention followed closely the ideas contained in the revolutionary leadership's concept of the distribution of wealth.³⁵

The revolutionary leadership's concept of the distribution of wealth also controlled the content of much of the debate between the political parties in the national congress. Most of the two-party systems that arose in the nineteenth century pitted one type of party that adhered to laissez-faire principles (the

³⁴ Tocqueville, *Democracy in America*, 2: 152; Pulszky and Pulszky, *White Red Black*, 3: 128; Chevalier, *Society, Manners, and Politics in the United States*, 418. Foreigners' observation of the American celebration of labor was also detected by Wood, *Radicalism of the American Revolution*, 285. It is not difficult to document the use of the labor theory of property or value by Americans in the nineteenth century because it is written on almost every document. Working-class leaders in the 1830s complained that other classes used the labor theory of value when it rightfully belonged to the workers. Doubters of the theory appeared, such as Francis Lieber and Amos Kendall, but they were exceptional. Political economists in the United States tended to be more reserved about the precise meaning of the labor theory of value and qualified more extensively what they meant by labor (mental and manual, as opposed to the working-class definition, which distinctly eliminated most of the mental labor), but they did not usually reject the concept. One of the differences between American and British political economists was that the British contingent, seeing working-class spokesmen use the labor theory of value as a means to reject the ethical underpinnings and results of capitalism, moved away from the theory. That was not true in America until the 1880s. See Meek, *Studies in the Labour Theory of Value*, 121–26.

³⁵ A good review of the relevant literature is Donald J. Pisani, "Promotion and Regulation: Constitutionalism and the American Economy," *Journal of American History*, 74 (December 1987): 740–68, esp. 744–54; see also James Willard Hurst, *Law and the Conditions of Freedom in the Nineteenth-Century United States* (Madison, Wis., 1956), 6–9, 27–28, 50–58, 90; Hurst, *The Legitimacy of the Business Corporation in the Law of the United States, 1780–1970* (Charlottesville, Va., 1970), 20–34; Stanley I. Kutler, *Privilege and Creative Destruction: The Charles River Bridge Case* (Philadelphia, 1971), 15–17, 22, 45–47, 117–18; L. Ray Gunn, *The Decline of Authority: Public Economic Policy and Political Development in New York, 1800–1860* (Ithaca, N.Y., 1988), chaps. 3, 4, 6.

Jefferson-Jackson Democratic party axis) against another type that favored governmental promotion of economic development (the Federalist-Whig-Republican party axis). Yet both party axes wished to avoid legislative favoritism to individuals and establishment of monopolies; all parties operated within the boundaries set by the American concept of the distribution of wealth. Those on the Jefferson-Jackson Democratic axis called for laissez faire and thereby demonstrated unwavering fidelity to republican fears arising from the political economy of aristocracy. The members of the Federalist-Whig-Republican axis tried to gain support for laws promoting general economic improvement that favored no person but that widened the field of opportunity for all; thus they, too, could claim to be acting against a political economy of aristocracy. The party positions on legislation also indicate how they differed on the distribution of wealth. Those on the Jefferson-Jackson axis desired a narrow range of wealth distribution, maintaining that deviations in mental and physical capabilities among men were not great enough to justify extremes in wealth holding. The Federalist-Whig-Republican axis members celebrated individual differences and thus justified a wider spread in the distribution of wealth. Nonetheless, Federalist-Whig-Republican politicians still insisted that wealth accumulations be the result of labor, not legislative favoritism or monopoly.³⁶

The most strident political use of the revolutionary generation's theory of the distribution of wealth occurred over banking and tariff issues. On these subjects, American politicians wielded the concept of the political economy of aristocracy on institutional development like Hercules wielded his club. John Taylor of Caroline, foe of the First Bank of the United States, openly fretted about positive government action upsetting the distribution of wealth by transferring the fruits of labors of producers to non-producers—credit systems were the principal means of erecting aristocracies and producing “tyrants and slaves—an aristocracy enormously rich, and a peasantry wretchedly poor.” Taylor's analysis was absorbed by Jacksonian Democrats, especially in the work of William M. Gouge. Andrew Jackson's famous veto message of the recharter of the Second Bank of the United States is one of the most succinct examples of mid-nineteenth-century application of the revolutionary theory of the distribution of wealth:

³⁶ The Federalist Party is something of an enigma on this point, as it appears to have been the first and only American party to have embraced publicly the hierarchical principle, even if was based on some sort of natural aristocracy. Alexander Hamilton is even more enigmatic; he appears not to have been worried about the distribution of wealth at all. Rather, Hamilton seems to have been about as pure an advocate of European mercantilism as the United States has ever produced; see, for example, John C. Miller, *Alexander Hamilton: Portrait in Paradox* (New York, 1959), 285–86, 290. On the stand of the party systems on economic issues, see Banning, *Jeffersonian Persuasion*, 138–49, 274–83; John C. Miller, *The Federalist Era, 1789–1801* (New York, 1960), 55–69, 79, 114–21; William Nisbet Chambers, *Political Parties in a New Nation: The American Experience, 1776–1809* (New York, 1963), 55–59, 100–02; John Ashworth, “Agrarians” and “Aristocrats”: *Party Political Ideology in the United States, 1837–1846* (Cambridge, 1983), 16–17, 23–24, 28–30, 39–41, 53–56, 61–65; Daniel Walker Howe, *The Political Culture of the American Whigs* (Chicago, 1979), 9, 19–21; Thomas Brown, *Politics and Statesmanship: Essays on the American Whig Party* (New York, 1985), 37–40; Rush Welter, *The Mind of America, 1820–1860* (New York, 1975), 77–89, 106–26, 130–62, 172–73; Harry L. Watson, *Liberty and Power: The Politics of Jacksonian America* (New York, 1990), 132–35, 167–71, 211–21, 243–47, 252; Lawrence Frederick Kohl, *The Politics of Individualism: Parties and the American Character in the Jacksonian Era* (New York, 1989), 58–61, 65–66, 119–20, 187–218.

It is to be regretted that the rich and powerful too often bend the acts of government to their selfish purposes. Distinctions in society will always exist under every just government. Equality of talents, of education, or of wealth can not be produced by human institutions. In the full enjoyment of the gifts of Heaven and the fruits of superior industry, economy, and virtue, every man is equally entitled to protection by laws; but when the laws undertake to add to these natural and just advantages artificial distinctions, to grant titles, gratuities, and exclusive privileges, to make the rich richer and the potent more powerful, the humble members of society . . . have a right to complain of the injustice of their Government.

And the Whigs really had no answer except the puny one of expediency. A national bank, they said, was useful to a commercial society. Not surprisingly, the establishment of the free banking system and then the national banking system were Democratic victories, not Whig or Republican ones.³⁷

The tariff presented more complications for nineteenth-century political antagonists than did banking issues. Certainly, the ideas opposing a political economy of aristocracy condemned the granting of privileges, exclusive rights, and monopolies, which was precisely the basis on which free traders and Democrats attacked high rates of duties.³⁸ Protectionists evolved an interesting counterweight to what seemed to be the logical reading of the tariff issue if one were in opposition to a political economy of aristocracy. Protectionists—and not free traders—were explicitly concerned about the distribution of wealth in the manner of the revolutionary generation.³⁹ From their dissection of economic activity came two notable conclusions. First, to preserve a republican distribution of wealth in a commercial society, it was necessary to ensure that the reward of labor (wages) was high. Second, aristocracies purposefully reduced wages to the starvation point in order to destroy in imperialist fashion the economies of other lands. To compete against a nation that purposely reduced wages was lunacy. If it had not been for the strange economy of the South and the political power of

³⁷ John Taylor, *An Examination of the Late Proceedings in Congress Respecting the Official Conduct of the Secretary of the Treasury* (Richmond, Va., 1793), 12; quote in Taylor, *An Enquiry into the Principles and Tendency of Certain Public Measures* (Philadelphia, 1794), 30; William M. Gouge, *A Short History of Paper Money and Banking in the United States to Which Is Prefixed an Inquiry into the Principles of the System* (1833; New York, 1968), 91; Andrew Jackson, "Veto Message," July 10, 1832, in James D. Richardson, ed., *A Compilation of the Messages and Papers of the Presidents, 1789–1902* (Washington, D.C., 1903), 2: 590. See also Robert E. Shalhope, *John Taylor of Caroline: Pastoral Republican* (Columbia, S.C., 1980), 5, 66–67, 77–81, 136, 153–54, 163–66; Major L. Wilson, *The Presidency of Martin Van Buren* (Lawrence, Kan., 1984), 71–75; Ashworth, "Agrarians" and "Aristocrats," 15–16, 43, 128.

³⁸ For example, Report of a Committee on the Memorial from Boston Merchants, *Annals of Congress*, 18th Cong., 1st sess., Appendix, 3081; John Taylor, *Tyranny Unmasked* (Washington, D.C., 1822), 19, 22, 67; Taylor, *Arator: Being a Series of Agricultural Essays, Practical and Political; In Sixty-Four Numbers*, 4th edn., M. E. Bradford, ed. (1818; Indianapolis, Ind., 1977), 93–94; speech of John Bell of Tennessee, June 8, 1832, *Register of Debates*, 22d Cong., 1st sess., 3348–78; Huston, *Panic of 1857 and the Coming of the Civil War*, 78.

³⁹ Free traders adopted the style of Malthus, Ricardo, and John Stuart Mill; their distribution theory became income shares to the factors of production, that is, wages, profits, rents, and they simply eliminated the old republican concern about the distribution of wealth. This was not the case for protectionists. If one wishes to find the echoes of republican concerns for the distribution of wealth among political economists, one will search for it in vain among free traders; it only survived in the works of protectionists.

southerners, the verdict would have been that the protectionists had the better republican and economic argument.⁴⁰

Among northerners, the revolutionary generation's understanding of the political economy of aristocracy played a pivotal role in dissecting, examining, and villifying the institution of slavery. As early as 1790, northerners had put into print the idea that slavery violated the labor theory of property or value, that slaveholders were aristocrats because they usurped the labor of others, and that slavery, by denying the worker his or her rightful reward, retarded economic growth. In 1793, for example, Noah Webster said of slavery that "to labor solely for the benefit of other men, is repugnant to every principle of the human heart," and that slaves, as normal human beings, would not work industriously "without a well founded expectation of enjoying the fruits of their labor."⁴¹ Americans in the last half of the eighteenth century had no trouble testifying to the aristocratic nature of the slaveholder—they lived off the labor of others. Quaker abolitionist John Woolman wrote that the sin of slaveholding was stealing another's fruits of labor so that the slavemaster "may live at Ease, and fare sumptuously, and lay up Riches for their Posterity." French visitor Brissot de Warville was more blunt: Virginian slaveholders "like to live off the sweat of their slaves" and were opposed to emancipation because it would mean "the plantation owner would have to work himself." As for the economic prospects of a slave-driven economy, early abolitionist David Rice commented, "slavery produces idleness; and idleness is the nurse of vice." Philadelphia physician Benjamin Rush judged the economic results of slavery by the light of the labor theory of property or value: the soil "seems to shrink into barrenness under the sweat of the slave." The error of slavery, said Rush, was that God commanded that man be free "that he might cultivate his possession with the sweat of his brow; but still should enjoy his Liberty." Moreover, Rush also knew that slavery produced an unhealthy and unrepugnant distribution of wealth.⁴²

⁴⁰ For example, Charles Hudson, "Protection of American Industry: Its Expediency and Necessity," in *Whig Almanac* for 1844, 25; H. G. O. Colby, "The Relations of Wealth and Labor," in Horace Greeley, ed., *The American Laborer, Devoted to the Cause of Protection to Home Industry* . . . (1843; New York, 1974), 233–38; John Pendleton Kennedy, *Letter of J. P. Kennedy to His Constituents, Citizens of the Fourth Congressional district in the State of Maryland, on the Principles and Values of the Protective System* (Baltimore, Md. [1842?]), 17; Francis Bowen, *The Principles of Political Economy* (Boston, 1856), 16–17; Daniel Raymond, *The Elements of Political Economy, in Two Parts*, 2d edn., 2 vols. (1823; New York, 1964), 1: 52–53 and 2: 15–16, 26, 38, 60–66, 71, 115–16, 119–20; James L. Huston, "A Political Response to Industrialism: The Republican Embrace of Protectionist Labor Doctrines," *Journal of American History*, 70 (June 1983): 41–48.

⁴¹ Noah Webster, *Effects of Slavery on Morals and Industry*, 22; see David Cooper, who quoted Locke's labor theory of value in denouncing slavery, in [David Cooper] *A Mite Cast into the Treasury: or, Observations on Slave-keeping* (Philadelphia, [1772]), 22–23; [Nathaniel Appleton], *Considerations on Slavery in a Letter to a Friend* (Boston, 1767), 4.

⁴² John Woolman, *Some Consideration on the Keeping of Negroes, Recommended to the Professors of Christianity of Every Denomination* (Philadelphia, 1754), 16; J. P. Brissot de Warville, *New Travels in the United States of America 1788*, Mara Soceanu Vamos and Durand Echeverria, trans., Durand Echeverria, ed. (Cambridge, Mass., 1964), 231; Philanthropos [David Rice], *Slavery Inconsistent with Justice and Good Policy* (Lexington, Ky., 1792), 17; [Benjamin Rush], *An Address to the Inhabitants of the British Settlements in America upon Slave-Keeping* (Philadelphia, 1773), 6–7. See also comments of Mrs. Mercy Warren, *History of the Rise, Progress and Termination of the American Revolution, Interspersed with Biographical, Political and Moral Observations*, 3 vols. (1805; New York, 1970), 1: 21–22; Banner, *To the Hartford Convention*, 85; Daniel J. McInerney, "'A State of Commerce': Market Power and Slave Power in Abolitionist Political Economy," *Civil War History*, 37 (June 1991): 101–19.

The revolutionaries' understanding of the distribution of wealth thus explains the two current major interpretations of the Republican Party of the 1850s and the coming of the Civil War: the free labor ideology and the slave power conspiracy. But the view that a slave as a worker was deprived of the fruits of his labor and the slavemaster as an aristocrat lived off the labor of others and protected his status by political power was not a product of the 1850s or the 1830s. The condemnation of slavery on the basis of the American theory of the distribution of wealth was complete by 1800, and that condemnation subsumed the ideas behind both the free labor ideology and the slave power conspiracy.⁴³

Southerners rapturously espoused political republicanism, especially its libertarian variant, but they encountered trouble with economic republicanism as defined in the revolutionary concept of the distribution of wealth. From the beginning, slaveholders found a variety of ways to defend slavery, but the most persistent was a reliance on the doctrine of property rights: slaves were property, and by revolutionary ideology an individual could not be deprived of property without due process of law.⁴⁴ The point of separation between northerners and southerners was the universality of the labor theory of property or value; southerners for racial reasons and sheer self-interest could not bring themselves to admit that Africans deserved the fruits of their labors. In fact, the proslavery contingent was emphatic on the point: great civilizations relied on a leisure class to advance society while being kept in comfort by a lower class performing all the drudgery.⁴⁵ Southern politicians found a ready use for a "fruits of labor" argument, but its presence could only have flabbergasted northern listeners, for its employment by slaveholders was an obvious irony, if not utter hypocrisy. One can only wonder what northerners thought when they read the "South Carolina

⁴³ For the slave power conspiracy theory, see Michael F. Holt, *The Political Crisis of the 1850s* (New York, 1978), 51, 151–54. For the free labor ideology, consult Eric Foner, *Free Soil, Free Labor, Free Men: The Ideology of the Republican Party before the Civil War* (New York, 1970), 11–39. William E. Gienapp makes the argument that the two interpretations are separable and that the more potent one was the slave power conspiracy; Gienapp, *The Origins of the Republican Party, 1852–1856* (New York, 1987), 356–57. But the free labor ideology and the slave power conspiracy are really two sides of the same coin, and the denomination of the coin is anti-aristocracy.

⁴⁴ For the defenses of slavery, see [Richard Nisbet], *Slavery Not Forbidden by Scripture; or, A Defense of the West-India Planters* (Philadelphia, 1773); St. George Tucker, *A Dissertation on Slavery with A Proposal for the Gradual Abolition of It, in the State of Virginia* (Philadelphia, 1796); David Brion Davis, *The Problem of Slavery in the Age of Revolution, 1770–1823* (Ithaca, N.Y., 1975), 148, 267–68, 340–41; Donald L. Robinson, *Slavery in the Structure of American Politics, 1765–1820* (New York, 1971), 70. For an example of the belief that political republicanism in the South was of the purest kind, see J. William Harris, "Last of the Classical Republicans: An Interpretation of John C. Calhoun," *Civil War History*, 30 (September 1984): 225–67.

⁴⁵ For example, Chancellor [William] Harper, "Slavery in the Light of Social Ethics," in E. N. Elliott, et al., *Cotton Is King, and Pro-Slavery Arguments: Comprising the Writings of Hammond, Harper, Christy, Stringfellow, Hodge, Bledsoe, and Cartwright, on This Important Subject* (Augusta, Ga., 1860), 551–52; E. N. Elliott, "Introduction," in *ibid.*, vii–viii; Robert L. Dabney, *A Defence of Virginia, [and through Her, of the South,] in Recent and Pending Contests, Against the Sectional Party* (New York, 1867), 276–78; George Fitzhugh and A. Hogeboom, *A Controversy on Slavery, between George Fitzhugh, Esq., of Virginia, Author of "Sociology for the South," etc., and A. Hogeboom, Esq., of New York* (Oneida, N.Y., 1857), 8–9; William Sumner Jenkins, *Pro-Slavery Thought in the Old South* (Chapel Hill, N.C., 1935), 73, 119–20, 125, 137, 285–91. For examples of southerners who realized that slavery violated the revolutionary creed of the fruits of labor belonging to the laborer, see Allan Galloway, "Origins of Slave-Holding Paternalism: The Bryan Family and the Great Awakening in the South," *Journal of Southern History*, 53 (August 1987): 381, 390–91.

Exposition and Protest," (1828) by John C. Calhoun. He complained that the tariff was the "means [by] which the fruits of our toil and labour, which on every principle of justice ought to belong to ourselves, are transferred from us to them [northern manufacturers]."⁴⁶ By revolutionary principles the fruits of labor belonged to the slave, not to the aristocratic slaveholder.

MOST POLITICIANS, NEWSPAPER EDITORS, AND CONTRIBUTORS to the written record found the American distribution of wealth quite egalitarian and admirable, but vigorous dissenters appeared early in the nineteenth century. Working-class spokespeople between 1829 and 1860 questioned the equity of the distribution of wealth in the United States and asserted that labor was not in fact getting its just reward; wrote one contributor to a reform journal, "Distributive justice is not a law of the present social order." Critics of the American economic system, such as Langton Byllesby, Orestes A. Brownson, Josiah Warren, and Thomas Skidmore, found the distribution of wealth in the country to be wretched. Most located the flaw in the system of wage dependency and so moved toward a radical criticism of the capitalist marketplace.⁴⁷ Yet the revolutionary concept of the distribution of wealth seems to have interfered with a radical solution to American wealth-holding patterns: Warren advocated "sovereignty of self" or anarchy, Byllesby argued for free western land (as of course did George Henry Evans), Brownson proclaimed the working-class remedy of laissez faire, and both Brownson and Skidmore promulgated redistribution of the wealth of those who died—that is, a massive tax on inheritances.⁴⁸ These authors created a radical criticism or analysis but propounded no radical solution.

The group that was most consistent in its radical understanding of the American distribution of wealth was the associationists, the Fourierites. Not only did they find the American distribution of wealth atrocious, the associationists also emphatically rejected the notions of the political economy of aristocracy. Albert Brisbane, the principal promoter of Fourierism in the United States, asserted,

⁴⁶ Calhoun Draft and Committee Report, "Exposition and Protest," in Robert L. Meriwether and W. Edwin Hemphill, eds., *The Papers of John C. Calhoun*, 17 vols. (Columbia, S.C., 1959–), 10: 456, 457; see Richard N. Current, *John C. Calhoun* (New York, 1966), 44–46. The most exemplary case of irony connected with slaveholding and the labor theory of property or value is John Taylor of Caroline; Taylor virtually screamed out the labor theory of property or value in his works but could never apply the doctrine to Africans; Taylor, *Arator*, 180–84; Shalhope, *John Taylor of Caroline*, 141–49.

⁴⁷ A Hand That Thinks, "Non-Producers," in (Albany) *Mechanic's Advocate*, June 26, 1847; Langton Byllesby, *Observations on the Sources and Effects of Unequal Wealth; with Propositions towards Removing the Disparity of Profit in Pursuing the Arts of Life, and Establishing Security in Individual Prospects and Resources* (1826; New York, 1961), 28, 32–34; Josiah Warren, *Equitable Commerce: A New Development of Principles . . .* (New Harmony, Ind., 1846), ii, 1–3, 12–17 [tract is mispaginated]; [Orestes A. Brownson], "The Laboring Classes," *Boston Quarterly Review*, 3 (July 1840): 364–77; Thomas Skidmore, *The Rights of Man to Property! Being a Proposition to Make It Equal among the Adults of the Present Generation* (1829; New York, [1966]), 3–4. See Edward Pessen, *Most Uncommon Jacksonians: The Radical Leaders of the Early Labor Movement* (Albany, N.Y., 1967), 141, 145–46, 153, 177; Helene Sara Zahler, *Eastern Workingmen and National Land Policy, 1829–1862* (New York, 1941), 23, 190–94.

⁴⁸ Byllesby, *Observations on the Sources and Effects of Unequal Wealth*, 6–23, 33–39, 40–57; Warren, *Equitable Commerce*, 10–11; Brownson, "Laboring Classes," 391–94; Skidmore, *Rights of Man to Property*, 60–61, 137–39, 155, 160–89, 270–357.

"The EVILS which afflict society are social, not political in their nature, and a Social Reform only can eradicate them." What was striking about the associationists was their willingness to go beyond a radical criticism of the distribution of wealth and attempt to fashion alternative social and economic structures. Viewing American society from the perspective of discourse over the distribution of wealth, the utopian reformers appear to have been the true radicals of their era.⁴⁹

The main current of American thought about the distribution of wealth, particularly in light of the axioms about the political economy of aristocracy, led quickly and inevitably to an embrace of laissez-faire principles. Historians have had severe problems in explaining the advocacy of laissez faire in the antebellum and postwar years. At most, certain historians have argued, radicals in the political parties trumpeted nothing more than equal opportunity, not equal conditions. The idea that political equality will produce an economic equality has almost drawn a collective guffaw from scholars.⁵⁰

The difficulty that twentieth-century scholars have experienced in understanding the nineteenth-century position on political equality, laissez faire, and the distribution of wealth has much to do with a reversal that occurred between 1880 and 1890 in perceptions about the causal relationships operating in society. Before 1900, inequality was thought to be accomplished by politics. And inequality was legislated into existence by the individuals who profited most from hierarchy and inequality—aristocrats. It was hardly illogical for Americans, given

⁴⁹ A Fourierist [Albert Brisbane], "On Association and Attractive Industry," *United States Magazine and Democratic Review*, 10 (January 1842): 31, 33; 10 (February 1842): 167. Carl Guarneri has recently stressed the radical quality of the associationists; Carl J. Guarneri, *The Utopian Alternative: Fourierism in Nineteenth-Century America* (Ithaca, N.Y., 1991), 4–6, 93–109.

⁵⁰ See Thomas Jefferson, First Inaugural Address, March 4, 1801, in Richardson, *Messages and Papers of the Presidents*, 1: 323; Andrew Jackson, "Veto Message," in *ibid.*, 2: 590; Gouge, *Short History of Paper Money and Banking*, 91; Theophilus Fisk, *An Oration on Banking, Education, &c. Delivered at the Queen-Street Theatre, in the City of Charleston, S.C., July 4th, 1837* (Charleston, 1837), 3–4; Samuel J. Tilden, "Divorce of Bank and State," 1838, in John Bigelow, ed., *The Writings and Speeches of Samuel J. Tilden*, 2 vols. (New York, 1885), 1: 86. For secondary literature trying to explain the explosion of laissez faire in the nineteenth century and the attitudes of political parties, see Douglas T. Miller, *The Birth of Modern America, 1820–1850* (New York, 1970), 118, 163–65; Edward Pessen, *Jacksonian America: Society, Personality, and Politics* (Homewood, Ill., 1969), chap. 3, and pp. 95, 202–29; Joseph L. Blau, "Introduction," to Blau, ed., *Social Theories of Jacksonian Democracy: Representative Writings of the Period, 1825–1850* (New York, 1947), xxiii–xxiv; Welter, *Mind of America*, 126, 132–41; Watson, *Liberty and Power*, 234–45; Marvin Meyers, *The Jacksonian Persuasion: Politics and Belief* (New York, 1960), 12, 186–93; Jean H. Baker, *Affairs of Party: The Political Culture of Northern Democrats in the Mid-Nineteenth Century* (Ithaca, N.Y., 1983), 144, 157; Robert V. Remini, *The Legacy of Andrew Jackson: Essays on Democracy, Indian Removal, and Slavery* (Baton Rouge, La., 1988), 40–43; Ashworth, "Agrarians" and "Aristocrats," 18–20; Kohl, *Politics of Individualism*, 109–19; Thomas Brown, *Politics and Statesmanship*, 37–39, 46; Howe, *Political Culture of the American Whigs*, 28, 34; Arthur M. Schlesinger, Jr., *The Age of Jackson* (Boston, 1949), 314–17, 336–38.

The secondary works on laissez faire in the postbellum era are much more acerbic: Paul F. Boller, Jr., *American Thought in Transition: The Impact of Evolutionary Naturalism, 1865–1900* (Chicago, 1969), 70–75; Richard Hofstadter, *Social Darwinism in American Thought*, rev. edn. (Boston, 1955), 5–6, 73, 83 and *passim*; Sidney Fine, *Laissez Faire and the General-Welfare State: A Study of Conflict in American Thought, 1865–1901* (Ann Arbor, Mich., 1956), 3–15, 32–44, 96–110, and *passim*; Schlesinger, *Age of Jackson*, 314–17, 336–38. Attempts have been made to turn the Whigs into economic regulators (see Howe, *Political Culture of American Whigs*, 34; Watson, *Liberty and Power*, 245) and Jacksonians into anti-capitalist New Dealers (Watson, *Liberty and Power*, 170; Wilson, *Presidency of Martin Van Buren*, 71–73; Schlesinger, *Age of Jackson*, 510–19). But the simple fact is that all political parties until the 1890s operated within the confines of a laissez-faire framework, the only true exception being, possibly, the Federalists.

their reference points, to *expect* that a policy of small government, no bureaucracy, and no favoritism would produce an equitable distribution of wealth. If the government were removed from economic affairs, Americans saw no other means by which aristocrats or would-be aristocrats could achieve a transference of the fruits of labor from producers to themselves.

Twentieth-century scholars have exactly reversed this perception of how inequality is generated. Ever since the Progressive movement, it has been standard in historical accounts to assert that capitalism produces inequality and a skewed distribution of wealth that only politics can rectify. The analysis has been completely turned around. The difference in perspectives can be illustrated by noting the contrasts between two phrases and the contexts that surround them: in the late eighteenth and nineteenth centuries, economic processes were explained by reference to "the political economy of aristocracy"; for those in the twentieth century, economic processes have been explained by reference to "the political economy of capitalism." Historians have been slow in recognizing how present analysis of economic functioning has obscured the expectations and analyses of those dwelling in the nineteenth century.

Obviously, the revolutionaries' concept of the forces affecting the distribution of wealth did not survive into the twentieth century. In fact, the death of the American understanding of the distribution of wealth occurred with amazing speed. At the start of the year 1880, the four revolutionary axioms of the distribution of wealth ruled supreme throughout the land; by 1900, those axioms were not only discarded, they were viewed as quaint at best, ludicrous at worst. The revolutionary concept of the distribution of wealth has clear birth and death dates: circa 1776 to circa 1889.⁵¹ The agent of its demise was the emergence of the large-scale corporation, the rise of Big Business, in the form of the trust movement in the 1880s and then in the merger wave of 1898–1904.⁵² Simply put, the corporation killed the republican theory of the distribution of wealth and probably ended whatever was left of the political theory of republicanism as well.

Under the corporation, the idea of the fruits of labor withered on the vine. The labor theory of property or value as applied in the United States was individualistic; the individual earned his bread by industry, frugality, initiative, and avoidance of luxury. The individual was independent. Now the corporation brought about a new form of dependency. Instead of industry, frugality, and initiative producing fruits, underlings in the corporate hierarchy had to be aware

⁵¹ Dating the origin of the theory seems obvious, although it could logically be pushed back to 1765 or even perhaps 1745. For a closing date based on written sources and not on structural changes, I would choose the date of publication of Andrew Carnegie's essay, "The Gospel of Wealth," in the *North American Review* of June and December 1889. (A reprint may be found in Andrew Carnegie, *The Gospel of Wealth and Other Timely Essays* [Garden City, N.Y., 1933].) In the first eight pages, Carnegie details what the new distribution of wealth would be, why it came to be so, and why the old ideas concerning the distribution of wealth, especially entail and primogeniture, were no longer valid; Carnegie, *Gospel of Wealth*, 1–8.

⁵² On the economic transformation and its societal impact, see John A. Garraty, *The New Commonwealth, 1877–1890* (New York, 1968), xiii, 28, 96–97, 107–09, 310–11; Robert H. Wiebe, *The Search for Order, 1877–1920* (New York, 1967), 45–53; Martin J. Sklar, *The Corporate Reconstruction of American Capitalism, 1890–1916: The Market, the Law, and Politics* (Cambridge, 1988), 20–33; Alfred D. Chandler, Jr., *The Visible Hand: The Managerial Revolution in American Business* (Cambridge, Mass., 1977), chaps. 7–14.

of style, manners, office politics, and choice of patrons—very reminiscent of the Old Whig corruption in England at the time of the revolution—what is today called “corporate culture.” Appearance, not performance, became the criterion for promotion and wealth. It is worth noting that with the rise of Big Business, the labor theory of property or value, except among laboring groups and small businesspeople, has virtually disappeared from public discourse.

Like the labor theory of property or value, the emphasis on entail and primogeniture as crucial factors in the distribution of wealth met its demise when large-scale enterprise emerged. Throughout the nineteenth century, entail and primogeniture were taken seriously as laws that enabled aristocrats to reap the fruits of others’ labors. In the twentieth century, those laws barely obtain the respect of a scholarly sneer. Entail and primogeniture have simply disappeared as a part of economic discourse.⁵³

Behind the passing of the older concepts of the republican theory of the distribution of wealth was a new understanding. The assumption on which the whole of the political economy of aristocracy had been founded was that the agent producing inequalities and aristocracies was politics. Most revolutionaries and their successors held the belief that, without the power of government, large-scale companies were a natural impossibility—the clearest example being the East India Company. Then suddenly in the 1880s and the 1890s, large-scale production units emerged. And they really did not rely on government. Their appearance was due to an evolution of the economy. The economic system, not the political system, became the source of an unequal distribution of wealth. As social reformer Richard T. Ely wrote in 1900, monopolies arose “from the nature of industrial society” and not—or rather, no longer—from “exclusive privileges expressly granted by the legislative branch of government.”⁵⁴ It was in the 1880s and 1890s that many Americans ruefully acknowledged that their forebears had analyzed the wrong system in attempting to understand how wealth distributions became skewed.

Once the perception became widespread that the economic system produced unacceptable wealth distributions, a new alignment on economic policies ensued, and the fault line became the understanding of monopoly. Monopoly had been the economic devil in republican distribution theory, and by 1900 it seemed as if monopoly were the only appropriate characterization of the American economy. Attitudes toward monopoly split into two camps. One group found that the new crop of large businesses, instead of restricting output, raising prices, and lowering wages (as traditional monopoly was supposed to do), was actually highly efficient and therefore worthy of continued existence and social and economic approval.⁵⁵ Moreover, these individuals seized on the doctrine of *laissez faire* and argued that

⁵³ For example, see Hugh McCulloch, *Men and Measures of Half a Century: Sketches and Comments* (New York, 1888), 436–37, 441, 443–46; speech of James B. Weaver, quoted in Fred Emory Haynes, *James Baird Weaver* (Iowa City, Ia., 1919), 148; speech of Henry Demarest Lloyd, “Revolution: The Evolution of Socialism,” October 6, 1894, in Charles McArthur Destler, *American Radicalism, 1865–1901: Essays and Documents* (New London, Conn., 1946), 214.

⁵⁴ Richard T. Ely, *Monopolies and Trusts* (New York, 1910 [1900]), 29.

⁵⁵ For example, Edward Chase Kirkland, *Industry Comes of Age: Business, Labor and Public Policy, 1860–1897* (1961; Chicago, 1967), 214–17.

even a private monopoly was better than government intervention; eventually, the market would right every wrong. This group became the twentieth century's conservatives.⁵⁶

The other group held to the old republican fear of monopoly. Monopoly was evil under any system of government because, private or public, it thrived on favoritism, market advantage, and unearned profits; it therefore had to be destroyed. Laissez faire had been the method of eliminating monopoly in the nineteenth century, a product of the republican theory of the distribution of wealth. But it failed to curb monopolization, and so this set of individuals, convinced that monopoly must be stopped, reversed laissez faire and insisted that government intervene in the economy and either destroy monopoly or regulate it in the public interest. From this camp came the socialists, the progressives, and the New Dealers—the birth of twentieth-century liberalism.⁵⁷

The rise of Big Business generated the most important transformation of American life that North America has ever experienced. The other reputed transformations are puny and uninteresting compared to the massive institutional reshuffling and the mental readjustment Americans made between 1880 and 1920. The rise of Big Business also created the modern views of the distribution of wealth—the outcome of a laissez-faire capitalist economic system. But until the 1880s, the revolutionary generation's understanding of the economic and political processes that governed the distribution of wealth ruled American economic thinking and influenced policy decisions for nearly one and a quarter centuries. The revolutionaries' concept of the distribution of wealth was at heart the understanding of an agrarian people about the economic forces operating in their lives; it was a fit theory for the Agrarian Republic. But the Agrarian Republic faded away at the opening of the twentieth century, and the revolutionaries' ideas about the distribution of wealth were meaningless to a society that had become the Industrial Empire.

⁵⁶ Notice the arguments about monopoly in Milton Friedman, with the assistance of Rose D. Friedman, *Capitalism and Freedom* (Chicago, 1962), 122–23, 129; and J. P. Gould and C. E. Ferguson, *Microeconomic Theory*, 5th edn. (Homewood, Ill., 1980), 392. More generally, see George E. Mowry, *The Era of Theodore Roosevelt and the Birth of Modern America, 1900–1912* (New York, 1958), 38–45; and Fine, *Laissez Faire and the General-Welfare State*, 96–104.

⁵⁷ For example, Destler, *American Radicalism*, 138, 158, 176; John Morton Blum, *The Republican Roosevelt* (1954; New York, 1962), 109–17; Ely, *Monopolies and Trusts*, 217–64; John Bates Clark and John Maurice Clark, *The Control of Trusts*, rev. edn. (New York, 1912 [1901]), vi, 1–7, 14–32; Louis M. Hacker and Benjamin B. Kendrick, with the collaboration of Helene S. Zahler, *The United States since 1865*, 4th edn. (New York, 1949), x; Mowry, *Era of Theodore Roosevelt*, 45–56; Fine, *Laissez Faire and the General-Welfare State*, 373–400; Sklar, *Corporate Reconstruction of American Capitalism*, 35–40, and chaps. 5–6.

The Troubled Origins of European Economic Integration:
International Iron and Steel and Labor Migration
in the Era of World War I

CARL STRIKWERDA

TWO WATERSHED YEARS, 1992, the European Community's attempt to achieve total economic integration, and 1989, the dramatic disappearance of the Iron Curtain, have created a new sense of Europe as an economic unit. Rather than orienting themselves to twenty-five individual national economies, business leaders, politicians, and investors have begun to adapt to a truly continental arena. Spain and Hungary compete now for the same investment funds; airlines and insurance companies scramble to find international partners; and professionals, stocks, and credit card information have begun moving across national borders in greater numbers than ever before.

In discussing how economic integration occurred in twentieth-century Europe, historians generally refer to two basic literatures. On the one hand, theoretical perspectives have portrayed integration as the natural, logical outcome of long-term economic trends. Once the industrial revolution made national economies dependent on large-scale imports and exports and financial transfers, it was inevitable that, eventually, business organizations and labor would also flow across borders, along with raw materials, goods, and capital investment.¹ On the other hand, specific studies have discussed how only after World War II, with the creation of the European Coal and Steel Community and Common Market, did meaningful integration in Western Europe take place.² The disjuncture between

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¹ Ali El-Agraa, "The Theory of Economic Integration," in *International Economic Integration*, El-Agraa, ed. (London, 1982); Peter Robson, *The Economics of International Integration* (London, 1980).

² Hans Schmitt, *The Path to European Union: From the Marshall Plan to the Common Market* (Baton Rouge, La., 1962); Uwe W. Kitzinger, *The Politics and Economics of European Integration*, 2d edn. (Westport, Conn., 1963); Pierre-Henri Laurent, "Historical Perspectives on Early European Integration," *Journal of European Integration*, 12 (1989): 89–100.

these two literatures is obvious but has rarely been explored. Why, if integration is a natural product of long-term trends created by the industrial revolution, did integration take so long to come about?

One answer, of course, is that integration is not simply the consequence of economic forces but has always been strongly influenced by political decisions. This article will look at both political and economic factors affecting European economic integration in the crucial period surrounding World War I. It will argue that a high degree of economic integration already existed in Western Europe on the eve of the war, a level not achieved again until at least the 1960s. Thus there were indeed economic forces pulling the European economy toward greater unity. World War I and its ripple effects, however, shattered the trend toward increasing integration in ways that we are only recently beginning to realize. The "1992" project by the European Community and the dismantling of the Soviet bloc are in one sense restoring the implicit unity of the pre-1914 world.

The implications of this thesis are four-fold. First, pre-1914 integration raises questions about the economic origins of World War I. Many scholars have portrayed European industry and government, especially in imperial Germany, as close allies.³ The internationalization of business described below, however, suggests that many business leaders saw peaceful ties between states as the best means to economic growth and were wary of programs for autarky proposed by governments and interest groups.

Second, the high level of economic integration before World War I also raises important questions about interpretive models used by many business and social historians to explain the pace and financial underpinnings of multinational development. Business historians, who hardly discuss World War I, have portrayed multinational corporations as evolving gradually until American leadership spurred their take-off in the 1950s.⁴ By contrast, another group of historians, largely though not exclusively Marxist, have argued that the early twentieth century marked a stage of so-called "finance capitalism," in which the great banks controlled industry. The war largely ended this stage of finance capitalism and thereby destroyed a basis for economic unity, which reemerged only under American hegemony.⁵ The multinational business ties between heavy industry in different European countries, explored in this article, raise problems with both of these interpretations. The development of European multinational corporations

³ Fritz Fischer, *War of Illusions: German Policies from 1911 to 1914* (New York, 1975); Hans-Ulrich Wehler, "Der Aufstieg des Organisierten Kapitalismus und Interventionsstaates in Deutschland," in *Organisierter Kapitalismus: Voraussetzungen u. Anfänge*, Heinrich August Winkler, ed. (Göttingen, 1974).

⁴ John Cantwell, "The Changing Form of Multinational Enterprise in the Twentieth Century," in *Historical Studies in International Corporate Business*, Alice Teichova, Maurice Lévy-Leboyer, and Helga Nussbaum, eds. (Cambridge, 1989); Alfred D. Chandler, "Technological and Organizational Underpinnings of Modern Industrial Multinational Enterprise," in *Multinational Enterprise in Historical Perspective*, Teichova, Lévy-Leboyer, and Nussbaum, eds. (Cambridge, 1986); Richard J. Barnett and Ronald E. Muller, *Global Reach: The Power of the Multinational Corporation* (New York, 1974).

⁵ Rudolf Hilferding, *Finance Capital: A Study of the Latest Phase of Capitalist Development* [1910], Tom Bottomore, ed. (London, 1981), has influenced writers from Lenin through Ernest Mandel, *Late Capitalism* (London, 1975), Giovanni Arrighi, *The Geometry of Imperialism: The Limits of Hobson's Paradigm* (London, 1983), esp. 121–48, and Kees van der Pijl, *The Making of an Atlantic Ruling Class* (London, 1984).

in heavy industry did not follow a slow, continuous path but expanded rapidly before World War I, only to contract in the 1920s and 1930s. Furthermore, industrial at least as much as finance capitalism created multinational economic ties before World War I. Many historians have overlooked the lead that industrialists in heavy industry increasingly took in foreign investment after 1900. Although severed by two world wars, the earlier international links forged by industrial capitalism provided an important precedent for the post-World War II search for European economic cooperation.

Third, the painful, punctuated quest for European unity shows how powerful a role governments and interest groups play in determining economic integration. Before the Great War, business and labor moved across borders easily because the state generally allowed private powers to act freely; since the early 1950s, Europe has been integrating its economies through deliberate state action.

Fourth, this thesis may help reconceptualize our periodization of twentieth-century history. Historians are professionally averse to prophecy, but other scholars may be forgiven for imagining that a century or more from now, the years from 1914 to 1989 may be seen as a painful period in which Europe groped to rediscover the cooperative route it had begun to take before World War I. If this is true, we may be on the verge of a new era, and it may repay us well to understand the roots of the era that is ending.

INTEGRATION ITSELF CAN BE UNDERSTOOD as a series of ascending levels. On the lowest level, goods, raw materials, and capital flow more or less freely depending on tariffs, quotas, or financial restrictions. This kind of integration first took place in the nineteenth century among most industrialized countries and formed the basis on which the European Common Market tried to expand in the 1950s. A more complex level of integration allows businesses to move their operations between countries or to merge with firms in other countries. Likewise, it permits labor to cross borders relatively easily. Because the European Community found itself moving only tentatively toward this level of integration, it began the project known as "1992."⁶

To demonstrate that this higher level of economic integration had begun to emerge before 1914, this article will look particularly at German, Belgian, and French companies in the iron and steel industry and at the corresponding movements of immigrant workers. The iron and steel industry provides an excellent case study of integration because it had enormous importance in 1900, with its large labor force, huge capital investment, and crucial control over armaments. Furthermore, iron and steel had close links to the essential energy sector of coal, as well as to finance, shipping, machine building, and the electrical industry. The world of labor, as well as that of business, formed a critical part of

⁶ A third level of integration would be monetary or economic union, in which countries internationalize control of the currency or economic policy. See Fritz Machlup, "A History of Thought on Economic Integration," in *Economic Integration: Worldwide, Regional, Sectoral; Proceedings of the Fourth Congress of the International Economic Association held at Budapest, Hungary*, Machlup, ed. (London, 1976), 61–85, as well as Robson and El-Agraa.

the story of these emerging multinationals. Heavy industrial firms that expanded across borders frequently employed immigrant workers, both at home and abroad. As in all industries, the decision to use immigrant workers was highly sensitive to both economic and political forces. As W. R. Böhring has remarked, "Demand [for immigrant workers] is caused economically, screened politically, and given effect to administratively."⁷

The traditional view of Europe before 1914 argues that a rising tide of aggressive nationalism forestalled cooperation between rival nation-states and led to war. One economic historian has stated, "In the forty years or so before the First World War, the tendencies that were breaking up the economic unity of Europe were getting stronger in relation to those that made for continuing integration."⁸ Most Continental countries adopted higher tariff levels against foreign goods, thus abandoning the free trade policies of the mid-nineteenth century.⁹ Coal-poor France, which had to import over a third of its coal, even imposed a duty on imported coal in order to keep prices up for domestic producers.¹⁰ Behind this concern of protecting home markets lay a fear of foreigners. The reactionary Action Française writer Léon Daudet claimed that German goods and business investments in France represented "Jewish-German espionage."¹¹ Despite rapidly growing exports, many Germans were convinced that their economy's future in world markets was threatened by opposition from foreigners.¹² Even Britain, which clung to free trade, experienced campaigns against "Made in Germany."¹³

Tariffs not only buttressed xenophobia and dampened international trade, they also encouraged economic interest groups to coalesce around nationalist policies. In Germany, big agricultural landowners (Junkers) and heavy industry agreed to tariffs protecting each other. They later offered mutual support for a large army, which the Junkers officered, and a navy, which heavy industry built.¹⁴ This "marriage of iron and rye" in Germany had its counterpart in the French "alliance of iron and wheat."¹⁵ Arno Mayer has broadened this argument to suggest that conservative elites dominated politics over almost all of Europe.¹⁶ These nationalist and protectionist policies often went hand-in-hand with imperialism. Colonies, it was believed, could buy goods and furnish raw materials that European countries might refuse to each other. Lenin was only one of the first of

⁷ W. R. Böhring, *Studies in International Labour Migration* (London, 1984), 140.

⁸ Sidney Pollard, *The Integration of the European Economy since 1815* (London, 1981), 59.

⁹ P. A. Gourevitch, "International Trade, Domestic Coalitions, and Liberty: Comparative Responses to the Crisis of 1873–1896," *Journal of Interdisciplinary History*, 8 (1977): 281–313.

¹⁰ François Crouzet, "Le charbon anglais en France au XIX^e siècle," in *Charbon et sciences humaines*, Louis Trénard, ed. (Paris, 1966).

¹¹ Léon Daudet, *L'Avant-Guerre: Etudes et documents sur l'espionnage juif-allemand en France depuis l'affaire Dreyfus* (Paris, 1913).

¹² Maurice Ajam, *Le problème économique franco-allemand* (Paris, 1914).

¹³ Ernest Edwin Williams, "Made in Germany," 5th edn. (London, 1897).

¹⁴ Alexander Gerschenkron, *Bread and Democracy in Germany* (Berkeley, Calif., 1943); Volker Berghahn, *Germany and the Approach of War in 1914* (London, 1973); Hans-Ulrich Wehler, *The German Empire, 1871–1918*, Kim Traynor, trans. (Dover, N.H., 1985). For a different view: Kenneth D. Barkin, *The Controversy over German Industrialization, 1890–1902* (Chicago, 1970).

¹⁵ Herman Lebovics, *The Alliance of Iron and Wheat in the Third French Republic, 1860–1914* (Baton Rouge, La., 1988).

¹⁶ Arno J. Mayer, *The Persistence of the Old Regime: Europe to the Great War* (New York, 1981).

a long line of writers who saw imperialism leading to world war.¹⁷ Meanwhile, the international labor union federations and the Socialist Second International failed to influence any diplomatic confrontation and, more devastating, had scant effect on popular nationalism.¹⁸ It is hardly surprising that the dominant interpretation of the pre-World War I era emphasizes nationalism, imperialism, and militarism. Or, as Norman Stone concluded in an excellent recent textbook, "After 1911, the war had already broken out in people's minds."¹⁹

Despite this portrait of nationalism and antagonism, it is not necessarily clear that Europe, especially Western Europe, was moving inevitably before 1914 toward conflict. Tariffs, although they had risen since the low levels of the 1860s, still remained much lower than they had been historically and lower than they were to be in the 1920s and 1930s. German tariffs in 1927 were, on average, over a third higher than they had been in 1913; by 1931, they had more than doubled. French tariffs—which were already higher than most—went up only slightly by 1927 but went up approximately 30 percent more by 1931.²⁰ West European tariffs in 1913 were also much lower than those in Russia and the United States.²¹ Nor did tariffs or nationalist agitation in Europe significantly dampen international trade: between France, Germany, Britain, and Russia, trade was at an all-time high and increased steadily in the last few years before World War I.²² The very flourishing of nationalist agitation against foreign goods came, in part, as a reaction to the growth of international trade—a growth that nationalist agitators largely failed to curtail.²³

Furthermore, the emphasis on nationalism and tariffs often overlooks the web of relationships between European states that certain activists created in order to *increase* internationalism. The French Ligue du Libre-Echange carried on a vigorous battle against protectionism, as did the Hansa-Bund in Germany.²⁴ The Comité Commercial Franco-Allemand and the Deutsch-Französischer Wirtschaftsverein worked actively with both countries' parliaments and consular officials for freer trade, lower railroad rates, and more uniform laws. In 1909, these two groups brought together members of the Chamber of Deputies and the Reichstag from Lorraine, mayors of towns on both sides of the border, and the

¹⁷ V. I. Lenin, *Imperialism: The Highest Stage of Capitalism* [1917] (New York, 1939).

¹⁸ Hans Mommsen, *Arbeiterbewegung und nationale Frage: Ausgewählte Aufsätze* (Göttingen, 1979); Milorad M. Drachkovitch, *Les socialismes français et allemand et le problème de la guerre, 1870–1914* (Geneva, 1953); Susan Milner, *The Dilemmas of Internationalism: French Syndicalism and the International Labour Movement, 1900–1914* (New York, 1990).

¹⁹ Norman Stone, *Europe Transformed 1878–1919* (Cambridge, Mass., 1983), 153.

²⁰ H. Liepmann, *Tariff Levels and the Economic Unity of Europe: An Examination of Tariff Policy, Export Movements and the Economic Integration of Europe, 1913–1931* (London, 1938), 383–401.

²¹ Gourevitch, "International Trade," 282–312; J. J. Pincus, "Tariffs," *Encyclopedia of American Economic History*, 2 vols., Glenn Porter, ed. (New York, 1980), 1: 439–49.

²² W. Ashworth, "Industrialization and the Economic Integration of Nineteenth Century Europe," *European Studies Review*, 4 (1974): 291–314; Raymond Poidevin, *Les relations économiques et financières entre la France et l'Allemagne de 1898 à 1914* (Paris, 1969), 769–73, 885–86; Paul Bairoch, "European Trade Policy, 1815–1914," in *The Industrial Economies: The Development of Economic and Social Policies*, *Cambridge Economic History*, vol. 8, Peter Mathias and Sidney Pollard, eds. (Cambridge, 1989), 88–90.

²³ C. Buchheim, "Aspects of XIX Century Anglo-German Trade Rivalry Reconsidered," *Journal of European Economic History*, 10 (1981): 289.

²⁴ Yves Guyot, *La jalousie commerciale et les relations internationales* (Paris, 1911); Siegfried Mielke, *Der Hansa-Bund für Gewerbe, Handel und Industrie 1909–1914* (Göttingen, 1976).

two nations' railroad directors to plan a new railroad line between French and German Lorraine.²⁵ Despite opposition from the military, which delayed this project, the political and economic support for it is striking.

PERHAPS THE MOST SIGNIFICANT INDICATOR OF A PERSISTENT TREND TOWARD international ties before 1914 is the rise of international business. Unilever, Royal Dutch Shell, and many U.S. multinationals had their roots in these years.²⁶ Many studies of multinational business imply that it is primarily a post-World War II phenomenon pioneered by U.S. corporations.²⁷ In terms of the businesses that existed across borders in the 1960s, there is some truth to this, but this notion overlooks the impressive growth up to 1914. Two world wars and the Great Depression destroyed the foreign holdings of so many European corporations that their growth after 1945 appears as virtually a new beginning. Many scholars have also argued that "finance capitalism" dominated this era. Large banks through indirect investments such as shares and loans are claimed to have controlled much of international business. Yet the level of direct foreign investment—that is, investment that carried significant management control as opposed to financial interest—was almost certainly higher in 1914 than at any time until the mid-1960s.²⁸ Direct investment by French and Belgians, for example, helped make Russia the world's fourth largest steel producer.²⁹ Britain was by far the greatest source of foreign investment; but, until recently, scholars have considered most of this investment as purely financial, that is, shares, bonds, and loans.³⁰ Much more of this investment, too, probably was direct.³¹ Although international finance had expanded greatly, it was not new. Direct investment by multinational firms, because it could integrate whole economies, was genuinely a break with the past.

It is this cosmopolitan world of international business that can give us an insight into the trends that the world war abruptly destroyed. The creation of interna-

²⁵ Consular reports, Paris to Berlin, July 13, 1908, R 85/64, and April 14, 1910, R 85/65, Abteilung II, Auswärtiges Amt, Bundesarchiv, Koblenz; "La percée des Vosges," *Journal des Débats*, April 28, 1909; Poidevin, *Les relations économiques*, 784–90.

²⁶ Charles Henry Wilson, *The History of Unilever: A Study in Economic Growth and Social Change*, 2 vols. (London, 1954); Anthony Sampson, *The Seven Sisters: The Great Oil Companies and the World They Made* (New York, 1975); Mira Wilkins, *The Emergence of Multinational Enterprise: American Business Abroad from the Colonial Era to 1914* (Cambridge, Mass., 1970).

²⁷ See note 4.

²⁸ Peter Svedberg, "The Portfolio-Direct Composition of Private Foreign Investment in 1914 Revisited," *Economic Journal*, 88 (December 1978): 763–77; John H. Dunning, "Changes in the Level and Structure of International Production: The Last One Hundred Years," in *The Growth of International Business*, Mark Casson, ed. (London, 1983). Because Svedberg, on whom Dunning relies, concentrates on Asia, Africa, and Latin America, intra-European direct investment may have been even higher than these authors suspect.

²⁹ John P. McKay, *Pioneers for Profits: Foreign Entrepreneurship and Russian Industrialization, 1885–1913* (Chicago, 1970); René Girault, *Emprunts russes et investissements français en Russie, 1887–1914* (Paris, 1973).

³⁰ Herbert Feis, *Europe, the World's Banker, 1870–1914* [1930] (New York, 1968), 26–32.

³¹ Mira Wilkins, "The Free-standing Company, 1870–1914: An Important Type of British Foreign Direct Investment," *Economic History Review*, 2d ser., 41 (1988): 259–82; John Stopford, "The Origins of British-Based Multinational Manufacturing Enterprises," *Business History Review*, 48 (Autumn 1974): 303–56.

tional business connections before World War I indicates the relative openness of the societies at the time. The reaction against such international ties and the failure of these ties to promote genuine cooperation, however, demonstrate the deep-rootedness of nationalism and the larger failure of the European political system to reconcile the power of nation-states with the need for economic integration.

The iron and steel industries on the West European continent—particularly Germany, France, Belgium, and Luxembourg—provide an interesting case study of international business before the war. Together, these were the most industrialized nations in the world after the United States and Britain. Iron and steel around 1900, more than at any other time, were the keys to economic and military power. Unlike Britain, the United States, or Russia, these nations lacked the right balance of iron ore and coal to be self-sufficient. France and Luxembourg had large reserves of iron but lacked coal. Germany and Belgium had coal reserves—Germany enormous ones—but needed to import iron.³² These western Continental European states, because of their proximity to Britain, could more easily integrate their economies than could many other regions. Even though British business played little direct role in these countries, its indirect role was immense: London provided the foundation for international monetary stability, inexpensive British coal and shipping benefited all coastal areas, and Britain and its empire were the largest and most open market for international trade.³³

Finally, the iron and steel industries in these countries became, along with those in nearby Italy and the Netherlands, the basis of today's European Community. In the 1950s, the European Coal and Steel Community and the European Economic Community partially re-created the relatively open Continental economy that had existed before 1914. Nevertheless, international business in pre-war heavy industry on the Continent has gone largely unexamined. There is a hardly a mention of it in any historical study of multinational business.³⁴ Studies of European economic integration treat trade, tariffs, labor migration, and international agreements on communications and transportation but largely ignore international business.³⁵ Even in the pre-World War I world, the importation of German goods drew more attention than did investment in foreign heavy

³² Fernand Maurette, *Les grands marchés des matières premières* (Paris, 1922), 1–16, 153–61; Norman J. G. Pounds and William Parker, *Coal and Steel in Western Europe: The Influence of Resources and Techniques on Production* (Bloomington, Ind., 1957), 127–246. By 1910, Britain imported approximately 30 percent of its iron ore; but, earlier, imports had been much less; and Britain's large foreign investments and merchant marine made it easier to obtain ore. British ore reserves were actually quite ample and were only not being worked because available technology did not yet make them competitive vis-à-vis easily obtained foreign ores. Henry Louis, "The Iron Ore Resources of the United Kingdom," in *The Iron Ore Resources of the World*, 2 vols. (Stockholm, 1910), 2: 623–41.

³³ Britain's import surplus with most of Continental Europe helped fuel other economies, while its goods still helped set the standard for a European-wide economy; Ashworth, "Industrialization"; Derek H. Aldcroft, ed., *The Development of British Industry and Foreign Competition, 1875–1914* (Toronto, 1968).

³⁴ Lawrence G. Franko, *The European Multinationals: A Renewed Challenge to American and British Big Business* (Stamford, Conn., 1976), 26–54, treats pre-World War I Continental European multinationals as largely propelled by the search for raw materials.

³⁵ See Pollard, *Integration*, 42–60, for example.

industry.³⁶ Yet the sheer size of these corporations and their political and military importance offer material for a revealing case study of internationalism. In this era, capital could move freely across borders, foreigners enjoyed almost all rights of incorporation, and multinationals could employ large numbers of foreign workers in their international branches. In many ways, it was these freedoms, too, that were lost after 1914 and that the European Community sought to re-create in the 1950s.

One key to the movement of heavy industrial companies across West European borders was the need for raw materials, especially iron. Because coal was much more abundant and so much of it needed to be burned in smelting, the iron and steel industry in almost every country grew up near coal fields and imported iron ore. In the 1860s, the scarcity of usable iron ore drove the biggest German and Belgian metallurgical producers to obtain mining concessions in Spain. Both the Krupp company, based in the Ruhr, and the John Cockerill company, based near Liège, purchased iron mines near Bilboa, Spain, which they were to continue to work into the twentieth century. In the 1870s and 1880s, however, the invention of the Thomas and Siemens-Martin processes for making steel made it possible to use high-phosphorus iron ores. The largest deposits of these in Western Europe were in Lorraine, on both sides of the Franco-German border, in Luxembourg, and in Sweden.³⁷ The ability to use these reserves allowed for a vast expansion of the iron and steel industry, as well as an expansion of coal mining to provide fuel. Eventually, the most successful iron and steel firms became those that were vertically integrated, that is, those that controlled their own sources of fuel and ore and could both smelt iron and produce steel and finished products.

As a result of the new processes, iron and steel firms sought iron ore fields in German and French Lorraine, Luxembourg, and, eventually, as the demand for steel skyrocketed, Normandy, Russia, and Morocco. For decades, laws in France and Luxembourg allowed foreigners to purchase mining concessions freely. By 1910, German and Belgian interests owned perhaps as much as 35 percent of the iron ore fields in French Lorraine, the largest iron ore field in Europe, and probably 40 percent of those in Luxembourg.³⁸ Almost all the major Ruhr industrial firms participated, including Thyssen, Hugo Stinnes's Deutsch-Luxemburg, and Emil Kirdorf's Gelsenkirchener. With capitalizations of over 100 million marks and labor forces of some 40,000 workers each, these were among the largest companies in the world. The largest Belgian iron and steel producer,

³⁶ Paul de Mirecourt, *Le commerce français aux mains des Allemands* (Paris, [1912?]); Buchheim, "Aspects," 289.

³⁷ Maurette, *Les grands marchés*, 157; Pounds and Parker, *Coal and Steel*, 116–23; David S. Landes, *The Unbound Prometheus: Technological Change and Industrial Development in Western Europe from 1750 to the Present* (Cambridge, 1969), 249–69; M. P. Nicou, "Les ressources de la France en minerais de fer," 1: 3–20, and G. Einecke and W. Kohler, "Die Eisenerzvorräte des deutschen Reiches," 2: 671–716, both in *Iron Ore Resources of the World*.

³⁸ Estimates vary because many firms owned concessions jointly or through subsidiaries: Michel Ungeheuer, "Die wirtschaftliche Bedeutung der ostfranzösischen Erz- und Eisenindustrie," *Technik und Wirtschaft*, 5 (1912): 11–15; and "Die wirtschaftliche Bedeutung der luxemburgischen Erz- und Eisenindustrie," *Schmollers Jahrbuch*, 40 (1916): 211–73; Dr. Kohlmann, "Die neuere Entwicklung des lothringischen Eisenerzbergbaues," *Stahl und Eisen*, 31 (March 16, 1911): 413–24.

Ougrée-Marihay from Liège, bought numerous concessions in France, as did Cockerill, Espérance-Longdoz, and Providence.³⁹

More revolutionary than the purchase of foreign iron ore concessions was these companies' decision to invest in iron and steel production in France and Luxembourg. In Luxembourg, laws compelled foreign owners of ore concessions to smelt iron within the duchy.⁴⁰ Also, it became more economical to bring coal to the iron mines and produce iron or steel there, rather than to bring iron ore to the coal fields. Technical advances dramatically lowered the amount of coal needed to smelt a ton of iron. Simultaneously, a "transportation revolution" occurred.⁴¹ Steam-powered iron and steel ships lowered the costs of moving both coal and iron, and corporations began to consider the best location on the basis of not only raw materials but labor costs and access to markets as well.⁴²

By 1912, German and Belgian heavy industry had taken over 90 percent of Luxembourg iron and steel and transformed itself in the process. Penetration by the Germans was facilitated because Luxembourg, although an independent state, belonged to the Zollverein, the German customs union. Deutsch-Luxemburg, the largest employer in the Ruhr after Krupp by 1913, was founded in 1901 when German companies combined with Belgian-owned Luxembourg firms.⁴³ Its close rival in the Ruhr, Kirdorf's Gelsenkirchener, produced even more iron in Luxembourg than did Deutsch-Luxemburg and nearly as much steel. The Belgian firm Ougrée-Marihay purchased a Luxembourg plant in Rodange in 1905.⁴⁴ In 1911, Belgian investors created ARBED (Acéries Réunies de Burbach, Eich, Dudelange) by combining three Luxembourg and German firms into one large corporation. Belgians controlled two-thirds of ARBED; the remaining capital was French, Luxembourgish, and German. ARBED also controlled iron ore fields in French Lorraine and iron and steel firms in the German Saar, making it larger than any other European heavy industrial concern except the largest Ruhr firms.⁴⁵ Luxembourg made an important contribution to German and Belgian industrial power; the tiny country was the world's sixth largest pig iron

³⁹ Claude Prêcheur, "Les liens financiers entre les industries des métaux de la France et de l'UEBL," in *Mélanges de géographie physique, humaine, économique, appliquée: Offerts à M. Omer Tulippe*, 2 vols. (Gembloux, 1967); Daniel Jacobs, "Gereguleerd Staal: Nationale en internationale economische regulering van de Westeuropese staalindustrie 1750-1950" (Ph.D. dissertation, Universiteit te Nijmegen, 1988), 210-15.

⁴⁰ Camille Wagner, *La sidérurgie luxembourgeoise* (Luxembourg, 1931), 20.

⁴¹ E. Levasseur, "Ports et marine de la France," *Revue économique internationale* (July 1911): 8-46; Kurt Wiedenfeld, *Die nordwesteuropäischen Welthafen . . .* (Berlin, 1903); Tsunehiko Yui and Keiichiro Nakagawa, eds., *Business History of Shipping: Strategy and Structure* (International Conference on Business History 11, Fuji) (Tokyo, 1984).

⁴² Wilhelm Pothmann, *Zur Frage der Eisen- und Manganerzversorgung der deutschen Industrie* (Jena, 1920), 22-24; Wilfried Feldenkirchen, *Die Eisen- und Stahlindustrie des Ruhrgebiets 1879-1914* (Wiesbaden, 1982), 84-85.

⁴³ *Recueil financier* (1910): 1115-16; Margareta Anna DeVos, *Kapitalverflechtungen in der Montanindustrie zwischen westlichen Deutschland und Belgien von etwa 1830 bis 1914* (Bonn, 1986), 207-10.

⁴⁴ Paul Spange, *Un siècle de hauts-fourneaux à Rodange 1872-1972* (Luxembourg, 1972).

⁴⁵ Félix Chomé, *Acéries Réunies de Burbach-Eich-Dudelange: Un demi-siècle d'histoire industrielle, 1911-1964* (Luxembourg, 1964), 18-47; Prêcheur, "Les liens financiers," 78-79; Pounds and Parker, *Coal and Steel*, 368-70.

producer in 1913 and may have been the world's fourth largest exporter of steel.⁴⁶

In France, Germans and Belgians went beyond investing in ore fields to taking over existing companies and changing the entire character of the French iron and steel industry. In 1903, Ougrée-Marihaye took control of the Chiers and Vireux-Molhain firms, while in 1908 Gelsenkirchener, with Belgian and Luxembourg investors, took over the Aubrives-Villerupt firm in the French Lorraine.⁴⁷ After 1900, Thyssen, Krupp, and the important Rotterdam merchant firms DePoorter and Muller opened up the Norman iron ore fields, which French capitalists had almost ignored.⁴⁸ Thyssen went further and, working with French companies, began building the largest steel mill in France in 1913 to process Norman iron ore. Thyssen's works near Caen took advantage of proximity to the ocean to bring in British, German, and French coal and to ship out finished steel.⁴⁹

Comparison with Sweden and Spain shows just how innovative this investment in French iron and steel was. Sweden had huge reserves of iron ore, yet there was almost no foreign investment in its iron and steel industry. Swedish law made it difficult for foreigners to obtain mining concessions, and the distance from coal reserves and the scarcity of labor may have also kept out investors in iron and steel. In 1913, Sweden exported only 200,000 tons of pig iron but 6,500,000 tons of iron ore.⁵⁰ Despite Britain's long history of investment in Spanish iron mining, and available coal from both Spanish and British mines, there was little integration of British iron and steel with Spanish iron ore sources. One of the most successful and profitable of British firms, the Consett Iron Company, integrated its direct ownership of Spanish iron mines with its coal mining and iron and steel production in Britain. Consett, however, remained the exception.⁵¹

⁴⁶ Wagner, *La sidérurgie luxembourgeoise*, 24; B. R. Mitchell, *European Historical Statistics 1750–1970* (New York, 1975), 224–25.

⁴⁷ Jean-Marie Moine, *Les barons du fer: Les maîtres de forges en Lorraine du milieu du 19^e siècle aux années trente* (Nancy, 1989), 76–77, 130; François Roth, "La participation des intérêts belges à l'industrialisation de la Lorraine (1860–1914)," in *Les relations franco-belges de 1830 à 1934* (Metz, 1975). In 1914, Aubrives-Villerupt had four French directors, three Belgians, three Luxemburgers, and one German—Heinrich Vehling, who served on the board (*Vorstand*) of Gelsenkirchener; *Recueil financier* (1915): 939, 848.

⁴⁸ Reports to Paul Reusch, 3001 93006/15, Historische Archiv der Gutehoffnungshütte, Oberhausen [hereafter, HA/GHH]; Jean de Maulde, *Les mines de fer et l'industrie métallurgique dans le département du Calvados* (Caen, 1916). Dutch capitalists invested in iron ore in order to have the flow of ore up the Rhine complement the shipment of coal down the river: H. P. H. Nusteling, *De Rijnvaart in het Tijdperk van Stoom en Steenkool 1831–1914* (Amsterdam, 1974), 305; "Société Française de Mines de Fer," *L'information* (March 19, 1912): 2.

⁴⁹ M. Ungeheuer, "Die Industriellen Interessen Deutschlands in Frankreich vor Ausbruch des Krieges," *Technik und Wirtschaft*, 9 (1916): 38–42; Wilhelm Treue, *Die Feuer verloschen nie: August Thyssen Hütte 1890–1926* (Düsseldorf, 1966), 153–59.

⁵⁰ B. Boethius, "Swedish Iron and Steel, 1600–1955," *Scandinavian Economic History Review*, 6 (1958): 143–75. Techniques in Swedish iron-making and bank financing in Sweden made foreign investment less promising: Theodor Sehmer, *Die Eisenversorgung Europas* (Jena, 1911), 55–87; Kurt Samuelson, "Banks and the Financing of Industry in Sweden, c. 1900–1927," *Scandinavian Economic History Review*, 6 (1958): 176–90.

⁵¹ M. W. Flinn, "British Steel and Spanish Ore: 1871–1914," *Economic History Review*, 2d ser., 8 (August 1955): 84–90; H. W. Richardson and J. M. Bass, "The Profitability of Consett Iron Company before 1914," *Business History*, 7 (July 1965): 71–92. Kenneth Warren, *Consett Iron, 1840 to 1980: A Study in Industrial Location* (Oxford, 1990), unfortunately does almost nothing with Consett's activities outside of Britain.

The German and Belgian investment in French and Luxembourg iron and steel, in other words, was not just a response to scarce raw materials but a contribution of managerial and entrepreneurial skills that raised the productive capabilities of the entire European economy. In 1909, German and Belgian steel production, for example, averaged 45,000 tons per blast furnace, the French 34,000, and the British 30,000.⁵² Since the domestic French market grew slowly, German and Belgian industrialists instead applied their expertise to the raw materials, skilled labor, and transportation facilities in France in order to export iron and steel products around Europe and the world.⁵³ Despite the arguments for "finance capitalism," this was not simply an extension of international banking or portfolio investment. France actually possessed more available capital than Germany, and capital exports were one of the France's few successful weapons in its economic competition with Germany.⁵⁴ French iron and steel firms, too, were renowned for their large capital reserves. Yet only where industry and the state took deliberate action to tie foreign loans to purchases of French goods did this abundant capital help French industry.⁵⁵ Otherwise, French capital went into government loans or was put at the disposal of foreign industry.⁵⁶ Even the ample capital reserves of French iron and steel firms probably reflected their caution rather than their success.⁵⁷ In Belgium and Germany, industrial firms and banks worked much more closely than in France to target investment in mining and manufacturing.⁵⁸ At the same time, in contrast to the "finance capitalism" argument, the giant German industrial corporations often freed themselves in these years from dependence on any one bank or group of banks.⁵⁹ Thus it was the managerial skills of industrial capitalism, and not simply the abundant funds

⁵² Robert C. Allen, "The Peculiar Productivity of American Blast Furnaces, 1840–1913," *Journal of Economic History*, 37 (1977): 625.

⁵³ Ann Wendy Mill, "French Steel and the Metal-Working Industries," *Social Science History*, 9 (1985): 307–38. Michael Jared Rust, "Business and Politics in the Third Republic: The Comité des Forges and the French Steel Industry, 1896–1914" (Ph.D. dissertation, Princeton University, 1973), 158–60, describes how Ougrée-Marihay's Gustave Trassenster nearly broke up the French rail cartel over obtaining an order to supply Tunisia.

⁵⁴ Poidevin, *Les relations économiques*, 723–26.

⁵⁵ This was particularly true in the armaments industry: Raymond Poidevin, "Fabricants d'armes et relations internationales au début du XIX^e siècle," *Relations internationales*, 1 (May 1974): 39–56.

⁵⁶ Karl Erich Born, *International Banking in the 19th and 20th Centuries* (Leamington Spa, 1983), 121–23.

⁵⁷ Maurice Vignes, "Le bassin de Briey et la politique de ses entreprises sidérurgiques ou minières," *Revue d'économie politique*, 27 (1913): 304–29.

⁵⁸ Hendricus Van der Valk, *De betrekkingen tussen banken en nijverheid in België* (Haarlem, 1932); Jacob Reisser, *The Great German Banks and Their Concentration* (Washington, D.C., 1911); Born, *International Banking*, 89–91.

⁵⁹ Gerald D. Feldman, *Iron and Steel in the German Inflation, 1916–1923* (Princeton, N.J., 1977), 19; Wilfried Feldenkirchen, "The Banks and the Steel Industry in the Ruhr: Developments in Relations from 1873 to 1914," *German Yearbook on Business History 1981*, 36–51. Germans and Belgians raised a good deal of French capital, often in Brussels or Alsace; Raymond Poidevin, "Weltpolitik allemande et capitaux français (1898–1914)," *Deutschland in der Weltpolitik des 19. und 20. Jahrhunderts*, Imanuel Geiss and Bernd Wendt, eds. (Düsseldorf, 1973); Born, *International Banking*, 123; Karl Strasser, *Die deutschen Banken im Ausland* (Munich, 1925), 12–90; R. Poidevin, "Les banques alsaciennes entre la France et l'Allemagne de 1871 à 1914," *Revue d'Allemagne*, 10 (1985): 479–89. The ability to raise French capital in Brussels, as well as the Latin Monetary Union, which put the Belgian and French francs at parity, may have encouraged Germans to work with Belgians.

of finance capitalism, that help to explain multinational investment in heavy industry.

BELGIAN AND GERMAN INVESTMENT IN LUXEMBOURG AND FRANCE began a process of internationalizing heavy industry in all four countries. ARBED, Ougrée-Marihaye's plant in Luxembourg, and Wendel—all Belgian and French firms—made up almost 20 percent of the German steel cartel, the Stahlwerksverband, while three of the six largest Ruhr members of the cartel—Gelsenkirchener, Deutsch-Luxemburg, and Thyssen—became heavily committed to investments in France, Belgium, or other countries.⁶⁰ Meanwhile, Belgian and German interests controlled perhaps 15 percent of the French iron and steel industry.⁶¹ Thyssen, because of his Caen steel mill, belonged to the business association of the French iron steel industry, the Comité des Forges. Gustave Träsenster, president of Ougrée-Marihaye and head of the Belgian steel cartel, joined the French rail cartel. Not surprisingly, the French, Belgian, and German rail and steel cartels reached a number of price-fixing agreements.⁶² By investing in French metallurgy, Belgians and Germans were even able to penetrate French cartels. The most important cartel in French iron and steel was the Comptoir Métallurgique de Longwy, which set prices and quotas for pig iron in the Meurthe-et-Moselle region. Since the region was the only one in France that produced a surplus of pig, it effectively controlled prices for all of France—as long as foreign importers cooperated. This cooperation arose, first, because Germans and Belgians invested in French firms and, second, because the French shared control of the Comptoir. In 1914, there were seventeen directors of the cartel: no less than eight represented firms either owned or associated with Germans or Belgians, even though these firms made up less than a quarter of the production of the cartel's members.⁶³

In trying to match the Belgian and German interpenetration of coal and iron across borders, French firms further internationalized heavy industry. Between 1904 and 1913, France moved from supplying only 5 percent of Germany's iron ore imports to supplying one-third. Germany, meanwhile, provided 80 percent of

⁶⁰ Feldman, *Iron and Steel*, 34–35.

⁶¹ If the production of Thyssen's new Caen works is considered at an annual rate, Chiers, Aubrives-Villerupt, Providence, Maizières-les-Metz, and Caen—all German or Belgian-owned—produced approximately 650,000 tons of pig iron, representing 12.5 percent of French production in 1913; while Providence and Caen produced over 10 percent of French steel. Calculated from Pounds and Parker, *Coal and Steel*, 368–70; Mitchell, *European Historical Statistics 1750–1970*, 217; "Die Werke von Caen," *Stahl und Eisen* (May 8, 1913): 784. There were a number of other foreign-owned firms.

⁶² Rust, "Business and Politics," 157, 175.

⁶³ The Conseil of the Comptoir included the president, chairman, and two directors of Aciéries de Longwy, which had formed a partnership with Röchling, two managers of Aubrives-Villerupt owned by Gelsenkirchener, the chairman of Chiers owned by Ougrée-Marihaye, and the chairman of the Belgian-owned Providence firm. Louis Launay and Jean Sennac, *Les relations internationales des industries de guerre* (Paris, 1932), 41–43; P. Obrin, *Le Comptoir métallurgique de Longwy* (Paris, 1908), 39, 50–58. This suggests that German industrialists did not expand to escape the hold of domestic cartels.

the coke imported into France.⁶⁴ In 1906, Aciéries de Longwy gave 50 percent control of a major Lorraine iron mine to the Röchling firm of the Saar in Germany. In exchange, Röchling gave the French firm one-quarter participation in a coal mine that it owned near Aachen.⁶⁵ Wendel, whose two interlocked firms straddled the Franco-German border, and Schneider, the smaller French rival to Krupp in armaments, invested in coal mines in the Netherlands, Belgium, and Germany, as did Pont-à-Mousson and Marine-à-Homécourt. Breaking with French tradition, Wendel raised capital on public markets in Germany, rather than relying simply on internal funds.⁶⁶

French investment in German, Belgian, and Dutch coal created a *de facto* alliance with the Ruhr-Westphalia coal and coke syndicates, which worked closely with the Belgian cartels and dominated the Dutch market.⁶⁷ Iron and steel was increasingly linked to the coal industry as more coal production came under the control of integrated works—metallurgical producers who owned or controlled their own coal sources. As the French Lorraine steel producer Aciéries de Micheville reported in 1910, “the German factory-mines [*mines-usines (Hüttenwerke)*] have conquered the majority of the direction of the [Ruhr] coal syndicate of Essen, and Micheville, like other establishments in the Meurthe-et-Moselle, finds itself placed from the point of view of fuel, in a state of subjection all the more dangerous as the company exports an important share of its products.”⁶⁸ Consequently, by 1913, Micheville, too, had invested in Belgian and German coal mines. Coal itself, however, was increasingly less important than coke—coal processed and hardened for use in smelting—and access to transportation between mines and blast furnaces. The Germans and Belgians had set up cokeries on the North Sea that could buy either British or German coal and ship coke to almost any iron and steel facility in Belgium, Luxembourg, or France.⁶⁹ Despite the concern of economic nationalists in France, who worried about industry’s dependence on foreigners, in 1911, five French iron and steel firms from Lorraine, with Belgian backing, combined to set up a cokery on the Dutch coast, using English and German coal to provide some of the same advantages that the Germans and Belgians enjoyed.⁷⁰

On the eve of the world war, the entire West European iron, coal, and steel

⁶⁴ Pothmann, *Zur Frage der Eisen- und Manganerzversorgung*, 95, 157.

⁶⁵ Hermann Kellenbenz and Jürgen Schneider, “Les investissements allemands en France, 1854–1914,” in *La position internationale de la France: Aspects économiques et financiers, XIX^e–XX^e siècles*, Maurice Lévy-Leboyer, ed. (Paris, 1977), 357.

⁶⁶ R. Poidevin, “Placements et investissements français en Allemagne, 1898–1914,” in Lévy-Leboyer, *La position internationale de la France*; Claude Fohlen, “Entrepreneurship and Management in France,” *Cambridge Economic History of Europe*, vol. 7 (Cambridge, 1978): 349–50; *François de Wendel 1874–1949* (n.p., [1949?]), 79; Claude Beaud, “La stratégie de l’investissement dans la société Schneider et Cie.,” *Entreprises et Entrepreneurs XIX–XX^e siècles* (Paris, 1983), 118–24; J. Tribot-Lasprière, *L’industrie de l’acier en France* (Paris, 1917), 224.

⁶⁷ Georges De Leener, *Le marché charbonnière belge* (Brussels, 1908), 67–76; Marcel Gillet, “Les charbonnages du Nord de la France face à la coalition germano-belge des producteurs de houille,” in his *Histoire sociale du Nord et de l’Europe du Nord-Ouest* (Lille, 1984).

⁶⁸ *Recueil financier* (1915): 1658–59.

⁶⁹ De Leener, *Le marché charbonnière belge*, 74; Maurette, *Les grands marchés*, 13–14; “Rombacher Hüttenwerke,” *Stahl und Eisen* (December 14, 1911): 2080–81.

⁷⁰ A.C.Z.C. (Sluiskuil, 1961); “Ein französisch-belgisches Kokerei-Unternehmen in Holland,” *Stahl und Eisen* (February 23, 1911): 330.

industry was becoming interdependent among its member nations. Through integration, Europe had the potential to eliminate the advantage the United States supposedly held from its abundant natural resources. As Gavin Wright has pointed out, Europe considered as a continent was no less well endowed; the real U.S. advantage lay in the absence of political boundaries dividing different regions.⁷¹ The multinational business connections that West European industrialists built up, both to obtain raw materials and to fill gaps left by firms that failed to innovate, had the potential to create an industrial economy as large as that of the United States.

Steel and iron was not exceptional in its degree of interpenetration. The growth of multinational oil and food-processing firms is relatively well known, but they may have done less to integrate Continental Western Europe than did firms in chemicals, metallurgy, and agriculture. In chemicals, Swiss and German firms, in addition to the Belgian firm Solvay, had operations throughout Europe.⁷² The largest German zinc producer owned a large company in Belgium, while the Continent's largest zinc firm, the French-Belgian multinational Vieille-Montagne, had over 13,000 employees scattered in Germany, Belgium, France, Britain, Italy, Algeria, Tunisia, Spain, and Sweden.⁷³ In agriculture, by 1906 Germany had already become second only to Britain as an importer of grain. German companies in Antwerp and Rotterdam handled much of this importation, which also fed Switzerland.⁷⁴ One reason the younger count von Moltke, director of German strategy, altered the original Schlieffen plan of 1895 was to ensure that the Netherlands would remain neutral and thus able to trade with Germany: "She must be the windpipe that enables us to breathe."⁷⁵

What made the international interpenetration of iron, coal, and steel doubly important was heavy industry's numerous links with other sectors. Because of its close links to electricity, mechanical construction, shipping, and chemicals, heavy industry could influence wide economic processes. Through its subsidiary Felten-Guilleaume, the German electrical giant AEG owned its own iron and steel plant in Luxembourg, and one of the largest mechanical construction firms in France was partly German-owned.⁷⁶ Norddeutscher Lloyd, the second largest shipping firm in Germany, invested in coal mines with Krupp, while Deutsch-Luxemburg invested heavily in steamship lines and ship building.⁷⁷ One of the major advantages that Belgian and German chemical firms had over French and British

⁷¹ Gavin Wright, "The Origins of American Industrial Success, 1879–1940," *American Economic Review*, 80 (1990): 651–68.

⁷² Paul M. Hohenberg, *Chemicals in Western Europe, 1850–1914: An Economic Study of Technical Change* (Baltimore, Md., 1967), 36–47; L. F. Haber, *The Chemical Industry during the Nineteenth Century* (Oxford, 1958), 9–35, 148–49, 170–80.

⁷³ *Recueil financier* (1915): 563–67, 852–54; Walter Hillman, *Die wirtschaftliche Entwicklung der deutschen Zinkindustrie* (Leipzig, 1911), 89–95, 112; Chambre Syndicale Française des Mines Métallurgiques, *Annuaire 1912–1913* (Paris, 1912), 188–96.

⁷⁴ Maurette, *Les grands marchés*, 62–63; Avner Offer, *The First World War: An Agrarian Interpretation* (Oxford, 1989), 86.

⁷⁵ L. C. F. Turner, "The Significance of the Schlieffen Plan," in *The War Plans of the Great Powers, 1880–1914*, P. Kennedy, ed. (London, 1979), 312.

⁷⁶ Wagner, *La sidérurgie luxembourgeoise*, 22, 181; Louis Bruneau, *L'Allemagne en France: Enquêtes économiques* (Paris, 1914), 236.

⁷⁷ Gaston Raphael, *Krupp et Thyssen* (Paris, 1925), 81; *Recueil financier* (1915): 1627.

competitors was the raw materials they obtained from the advanced coking processes used by Belgian and German heavy industry. Thyssen and Solvay even built a coking plant together, Thyssen to obtain coke for iron smelting, Solvay to acquire chemicals.⁷⁸ The need for iron and steel finally drove the German machine-building firm Mannesmann to purchase both a rolling mill and a coal mine, at the same time as it invested in countries all over Europe and tried to obtain iron ore concessions in Morocco.⁷⁹

THE MOVEMENT OF IMMIGRANT WORKERS BEFORE WORLD WAR I was both a prerequisite for the growth of international business in heavy industry and a clear demonstration of the relative openness of the international economy before the war. Entrepreneurial investment across borders appears all the more intriguing given that German and Belgian industrialists often had to import a large part of the labor they employed in France. Thanks to a precociously declining birth rate, France had the slowest population growth in the industrial world. The persistence of artisanal labor and subsistence farming also kept labor out of the industrial sector. In 1912, immigrants made up 57 percent of the population in the industrial area of French Lorraine and an even larger part of the labor force. Two-thirds of the industrial work force was Italian.⁸⁰ When Thyssen built his blast furnaces in Normandy, his companies employed a polyglot work force that included Italians, Greeks, and Moroccans.⁸¹ Nor was this foreign labor cheap, since during the economic boom before the war the United States and Argentina were often competing for the same workers. Germany itself employed approximately a million foreign workers.⁸² French labor contractors were outraged when a trainload of Italians whom they had hired stopped en route in Metz, in German Lorraine, and German recruiters lured away the entire contingent.⁸³

The dependence of multinational enterprise on an internationalized work force suggests interesting comparisons with firms in the late twentieth century that employ immigrant labor and move production to avoid high wages, strong unions, or generous welfare benefits. Wages in industrial areas in France and Luxembourg were generally higher than those in Belgium and only slightly lower than those in Germany. Many Belgians employed in French border towns lived in Belgium in order to take advantage of higher wages in France and lower costs in Belgium.⁸⁴ Unionization, too, was only slightly higher in Liège and the Ruhr than

⁷⁸ Hohenberg, *Chemicals in Western Europe*, 113, 126; DeVos, *Kapitalverflechtungen*, 239.

⁷⁹ Horst A. Wessel, *Kontinuität im Wandel: 100 Jahre Mannesmann, 1890–1990* (Düsseldorf, 1990), 144–48; Helmut Pogge v. Strandman, "Rathenau, die Gebrüder Mannesmann und die Vorgeschichte der Zweiten Marokkokrise," Geiss and Wendt, *Deutschland in der Weltpolitik des 19. und 20. Jahrhunderts*.

⁸⁰ Vignes, "Le bassin de Briey," 685–86.

⁸¹ "Übersetzung einer der Briefe des Herrn Le Chatelier vom 29 Okt. 1913," 3001 93006/5, HA/GHH.

⁸² Klaus Bade, "German Emigration to the United States and Continental Immigration to Germany in the Late Nineteenth and Early Twentieth Centuries," *Labor Migration in the Atlantic Economies: The European and North American Working Classes during the Period of Industrialization*, Dirk Hoerder, ed. (Westport, Conn., 1985), 133.

⁸³ Vignes, "Le bassin de Briey," 684.

⁸⁴ On wages: Hermann Aubin and Wolfgang Zorn, eds., *Handbuch der deutschen Wirtschafts- und*

in Luxembourg and the French Lorraine. Thus it seems unlikely that multinationals moved to avoid either high wages or unions. Although the German companies provided the most extensive system of welfare benefits, French and Belgian heavy industry also used costly benefits to recruit and control workers.⁸⁵

Rather than fleeing from their native workers, Belgian and German industrialists may have used their long experience in employing immigrants and workers who spoke a different language to facilitate multinational expansion. As early as 1876, almost a quarter of the work force around Liège, the home base of Ougrée-Marihaye, was non-native: 9 percent were Germans, and 13 percent were Dutch-speaking Flemings from northern Belgium.⁸⁶ Ruhr industrialists began employing Polish miners in large numbers during the 1880s, almost all of them German citizens from east Prussia, Posen, or Silesia.⁸⁷ By 1910, at least one-quarter of Ruhr miners were Polish, either German citizens or foreigners from Austria-Hungary or Russia, while many others were Italian or Dutch.⁸⁸ Experience with this diverse and multi-lingual work force may have made it easier for Belgian and German firms to expand to Luxembourg and the French Lorraine, where a majority of the industrial workers were non-native. The same may have been true for French firms. *Acéries de Longwy*, which later invested in German coal mines, in 1907 had a work force that was over 85 percent non-French—Belgians, Germans, Italians, and Swiss.⁸⁹ Thus multinational enterprise in iron, coal, and steel in Continental Europe went along with an internationalized work force.

A widespread perception exists that immigrants usually serve as an easily exploited "reserve army" of surplus labor.⁹⁰ Yet a significant number of foreign

Sozialgeschichte, vol. 2 (Stuttgart, 1976): 226–27; Jochen Krengel, *Die deutsche Roheisenindustrie 1871–1913* (Berlin, 1983), 91–101; "Reports of the British Board of Trade on Cost of Living in England and Wales, Germany, France, Belgium, and the United States," *Bulletin of the Bureau of Labor*, 93 (1911): 557–70; Ajam, *Le problème économique*, 35–37. On the border towns, *Chambre des Députés, Enquête sur l'état de l'industrie textile*, 5 vols. (Paris, 1906), 2: 184; Julian Thys and Olivier Vanneste, *Een analyse van de Grensarbeiders in Noord-Frankrijk* (Brugge, 1969), 36.

⁸⁵ E. Domansky-Davidsohn, "Der Grossbetrieb als Organisationsproblem des Deutschen Metalarbeiter-Verbandes vor dem Ersten Weltkrieg," *Arbeiterbewegung und industrieller Wandel*, Hans Mommsen, ed. (Wuppertal, 1978); Krengel, *Die deutsche Roheisenindustrie*, 32; C. Strikwerda, "Interest Group Politics and the International Economy: Mass Politics and Big Business Corporations in the Liège Coal Basin, 1870–1914," *Journal of Social History*, 25 (1991): 277–308; G. Noiriel, "Du 'patronage' au 'paternalisme': La restructuration des formes de domination de la main-d'oeuvre ouvrière dans l'industrie métallurgique française," *Le mouvement social*, 144 (1988): 17–36.

⁸⁶ J. M. Wautelet, "Accumulation et rentabilité du capital dans les charbonnages belges, 1850–1914," *Recherches économiques de Louvain*, 41 (1975): 79.

⁸⁷ Klaus Tenfelde, *Sozialgeschichte der Bergarbeiterschaft an der Ruhr im 19. Jahrhundert* (Bonn, 1977), 238–46.

⁸⁸ Christoph Klessmann, *Polnische Bergarbeiter im Ruhrgebiet 1870–1945* (Göttingen, 1978), 265–66; John J. Kulczycki, "Scapegoating the Foreign Workers: Job Turnover, Accidents, and Diseases among Polish Coal Miners in the German Ruhr," *The Politics of Immigrant Workers: Labor Activism and Migration in the World Economy since 1830*, Camille Guerin-Gonzales and Carl Strikwerda, eds. (New York, 1993), 133–54; Ulrich Herbert, *A History of Foreign Labor in Germany, 1880–1980: Seasonal Workers, Forced Laborers, Guest Workers*, William Templer, trans. (Ann Arbor, Mich., 1990), 21.

⁸⁹ Alphonse Merrheim, "L'Organisation patronale en France," *Le mouvement socialiste* (1908): 16.

⁹⁰ Gary S. Cross, *Immigrant Workers in Industrial France: The Making of a New Laboring Class* (Philadelphia, 1983), 10; Michael J. Piore, *Birds of Passage: Migrant Labor and Industrial Societies* (Cambridge, 1979); Stephen Castles and Godula Kosack, *Immigrant Workers and Class Structure in Western Europe* (London, 1973).

workers in this period were skilled, which suggests that, in the pre-1914 era, the labor market between West European countries functioned with some freedom. In 1913, the subprefect of Briey in Lorraine reported that the Marine-à-Homécourt firm recruited "a number of its engineers and the larger part of its foremen from the other side of the border"—Germany or Luxembourg.⁹¹ Far from being exceptional in its employment of immigrants, heavy industry even modeled itself in some cases on other sectors, particularly agriculture. Poles and Italians worked the harvest in Germany, while French sugar-beet farmers depended on thousands of Flemish Belgian migratory workers, known as "Franschmans."⁹² When the French iron and steel association, the Comité des Forges, wanted to systematize the recruitment of foreign labor, particularly Italians, it borrowed from the agreements made between Italy and the German government agency that coordinated the employment of agricultural workers, the Feldarbeitzentrale.⁹³

Becoming multinational did not necessarily make firms more conciliatory toward labor; some may have used their diversified position against workers. In 1911, Stinnes's Deutsch-Luxemburg tried to break a strike in Dortmund by hiring workers in Essen, ostensibly for its establishments in Luxembourg, and then bringing them into the strike-bound mill in Dortmund.⁹⁴ Some multinationals hired disproportionate numbers of immigrant workers for their branches in foreign countries, perhaps because they preferred to hire their own country's workers or at least not to employ labor of the host country. At the Frederick-Heinrich coal mine near Aachen, Germany, owned by the French firm Wendel, an observer noted, "Very few of the workers are Germans; one finds a mélange of Poles, Hungarians, Belgians, Dutch, Italians, and Swiss."⁹⁵ In Luxembourg, Gelsenkirchener hired proportionately more Germans and Ougrée-Marihaye more Belgians than did other firms.⁹⁶ Because the Luxembourg and Lorraine mines and steel mills lay right on the French, German, and Luxembourg borders, and because all of these establishments relied on immigrant labor, a large pool of Italian, Belgian, German, and French workers moved back and forth. In 1906, French metallurgists charged that employers in all three countries conspired to break strikes on the border.⁹⁷

The heavy demand for labor gave foreign workers bargaining power, however, and this may have encouraged some employers to improve conditions as an

⁹¹ Sous-préfet to Préfet, July 19, 1913, 4M 219/2, Archives Départementales de Meurthe-et-Moselle, Nancy.

⁹² M. Lair, "Les ouvriers étrangers dans l'agriculture française," *Revue économique internationale*, 4 (1907): 531–35; Luc Schepens, *Van Vlaskuter tot Franschman* (Brugge, 1973).

⁹³ Serge Bonnet, *et al.*, "Les Italiens dans l'arrondissement de Briey avant 1914," *Annales de l'Est*, 5^e série, 13 (1962): 83.

⁹⁴ David Crew, "Steel, Sabotage, and Socialism: The Strike at the Dortmund 'Union' Steel Works in 1911," *The German Working Class, 1888–1933: The Politics of Everyday Life*, Richard J. Evans, ed. (London, 1982), 129. Deutsch-Luxemburg had acquired Dortmund Union in 1910.

⁹⁵ Victor Cambon, *Les derniers progrès de l'Allemagne*, 3d edn. (Paris, 1914), 25.

⁹⁶ Deutsch-Luxemburg had the most heavily Italian work force. The Belgian-controlled but more locally managed ARBED employed the largest proportion of Luxemburgers; Ministère du Travail 52, Archives de l'Etat, Luxembourg; Serge Hoffmann, "L'immigration dans la tourmente de l'économie (1913/1940)," *Galerie*, 3 (1989): 339–43.

⁹⁷ "L'Alliance provisoire Franco-Allemande-Luxembourgeoise contre les grévistes métallurgistes de l'Est," *L'ouvrier métallurgiste* (May 1, 1906): 2.

alternative to repression. After 1905, French industrialists in Lorraine increased their wages and benefits and enticed a large number of Italian workers to move over the border from Germany.⁹⁸ Gelsenkirchener appears to have introduced welfare benefits that were standard in Germany into the Aubrives-Villerupt firm that it purchased in France.⁹⁹ The floating labor pool forced French, German, and Luxembourg firms to adopt similar wage scales and working conditions to attract workers. When implementation of the eight-hour day began to be discussed seriously, French, Belgian, and German representatives considered an international agreement to lower hours simultaneously.¹⁰⁰ Workers, furthermore, could in some cases use the international connections forged by multinationals and cartels to learn new techniques of labor organizing. When German metallurgists tried to organize workers in the Luxembourg plants of the multinationals, they worked with French and Italian unionists in order to reach Italians who drifted back and forth across the border. Union pamphlets sometimes came out in three languages—German, French, and Italian.¹⁰¹ In 1911, the Belgian metallurgical unions in Liège reorganized themselves after sending a study group to learn German union methods.¹⁰² The French metallurgist union leader Alphonse Merrheim repeatedly said that French iron and steel firms needed the challenge posed by the Germans to force them to modernize and, perhaps, to improve conditions for the workers.¹⁰³

By 1914, then, there were significant signs of an emerging European economy, embracing both business and labor, in one of the world's most important industries. Despite the upsurge of nationalist feeling after the second Moroccan crisis of 1911 and the tensions in Germany over Lorraine created by the Zabern incident of 1913, the interpenetration of the border regions went on unabated. On the eve of the war, French, German, and Luxembourg authorities were planning the construction of a tramway system connecting the three countries, to aid both workers and consumers in crossing the border.¹⁰⁴ How far this integration would have proceeded if war had not broken out is impossible to say, but it certainly had the momentum on the eve of the war to bring nations much closer together. Rather than seeing nationalism and hostility as the wave of the future, it may be better to see European society as uneasily balancing two divergent trends: one toward economic integration, which had the potential at least to encourage further internationalism, and the other toward self-interested and destructive nationalism.¹⁰⁵

⁹⁸ Bonnet, "Les Italiens," 87.

⁹⁹ Moine, *Les barons du fer*, 326.

¹⁰⁰ Rust, "Business and Politics," 240–42.

¹⁰¹ J 76/70, Justice, Archives de l'Etat, Luxembourg.

¹⁰² Centrale des Métallurgistes de Belgique, Province de Liège, Rapport du Comité Exécutif au Congrès du 2^{ème} Semestre 1913, 8.

¹⁰³ Merrheim, "L'Organisation," 190–93.

¹⁰⁴ "Aktien-Gesellschaft für Bahn-Bau und Betrieb in Frankfurt/a/M. Elektr. Strassenbahn von Esch über Deutsch-Oth bei Villerupt," 15 AL 588, Archives Départementales de la Moselle, Metz; David Schoenbaum, *Zabern 1913: Consensus Politics in Imperial Germany* (London, 1982).

¹⁰⁵ John L. Gaddis has argued a similar case for the contemporary world; "Toward the Post-Cold War World: Structure, Strategy, and Security," *Foreign Affairs*, 70 (1991): 102–22.

THE NATIONALISM PUSHING EUROPEANS TOWARD WAR and the reaction against economic interdependence were closely woven together. In 1911, a French newspaper claimed that a Belgian company was investing in the iron and steel industry as part of Krupp's plot to weaken France.¹⁰⁶ The Wendel family, which staunchly upheld their French identity after Germany annexed their establishments in Alsace-Lorraine, nonetheless tried to have France and Germany agree to link the Wendel plants in the two countries via a new rail line. Nationalist and military opinion doomed the project.¹⁰⁷ Thyssen's purchases of ore fields helped to prompt a change in French mining law against foreign purchases.¹⁰⁸ Writing in 1912, the French law professor Maurice Vignes worried about the threat posed by Italian labor in an industry that was critical for national defense and based right on the border: "If a war breaks out with Germany, what will prevent Italy from throwing this mass of workers against us?"¹⁰⁹ Reportedly, one reason Ruhr industrialists hesitated to invest more in steel and iron-making in either French or German Lorraine was the fear that the establishments would be directly in the path of any war.¹¹⁰

German industrialists themselves undercut the logic of international economic integration by playing a dangerous game of supporting, largely for domestic reasons, militarists and reactionary nationalists like the Pan-Germans. Ruhr businessmen wanted the support of the conservative elite and the Pan-Germans to forestall the growing power of labor and Socialism; they failed to see that these same groups could destroy Germany's international economic position.¹¹¹ The alliance of German industry with conservative nationalism, however, should not be taken out of context.¹¹² Despite their subsidies for the Pan-Germans and their support for the imperial government, many industrialists in their day-to-day business decisions were moving toward a Europe internationalized by contract, not conquest. By 1914, this process of internationalization had not gone far enough to force German industrialists to choose between national chauvinism and international cooperation. But, had the war not broken out, such a decision almost certainly would have been necessary in the near future.

The alliance between industrialists and radical nationalists in Germany was especially dangerous, nonetheless, because it showed how little importance industrialists attached to the need for a political alliance in favor of international

¹⁰⁶ Firmin Lentacker, *La frontière franco-belge* (Lille, 1974), 169.

¹⁰⁷ R 85/64, Auswärtiges Amt, Bundesarchiv, Koblenz; "Joeuf dans la vallée de l'Orne," Bibliothèque Municipale de Nancy, 142.

¹⁰⁸ "Décret du 14 janvier 1909 réglementant l'exploitation des mines," appendix to *Annuaire* 446; Kellenbenz and Schneider, "Les investissements allemands," 358; Rust, "Business and Politics," 215–16.

¹⁰⁹ Vignes, "Le bassin de Briey," 5. Italy was still nominally a member of the Triple Alliance with Germany and Austria-Hungary.

¹¹⁰ Feldenkirchen, *Die Eisen- und Stahlindustrie*, 86.

¹¹¹ Hartmut Kaelble, *Industrielle Interessenpolitik in der wilhelminischen Gesellschaft* (Berlin, 1967), 146–63; Elaine G. Spencer, *Management and Labor in Imperial Germany: Ruhr Industrialists as Employers, 1896–1914* (New Brunswick, N.J., 1984), 139–47; Roger Chickering, *We Men Who Feel Most German: A Cultural Study of the Pan-German League, 1886–1914* (Boston, 1984), 227–30; John A. Leopold, *Alfred Hugenberg: The Radical Nationalist Campaign against the Weimar Republic* (New Haven, Conn., 1977), 2–11.

¹¹² Hans Jaeger, *Unternehmer in der deutschen Politik (1890–1918)* (Bonn, 1967), 132–42, points out employers' ambivalence toward the Right.

integration. The arguments against imported goods, foreign investments, and multinational companies attracted the support of the lower middle class and farmers who already suffered from the downturns and harsh competition present in a capitalist system. Nationalist opposition to international economic integration was a convenient way for these groups to express their grievances against social and economic change under the banner of patriotism.¹¹³ If the benefits of economic integration were eventually to be enjoyed, and, even more important, if economic justifications were not to be harnessed to the desire for war, their grievances would have to be addressed.

The tension between the two trends, one toward internationalization, the other toward nationalism and protectionism, inevitably raises questions about the origins of World War I. Which trend was growing stronger in the last years before July 1914: peaceful integration or nationalist militarism? Few writers have taken heavy industry's multinational ties into account in the debate on the war. Both the pre-war idealists who argued that finance capitalism would guarantee peace and the muckrakers who accused armaments manufacturers of causing the war seem to have been ignorant of how deeply heavy industry in Continental countries had become interdependent.¹¹⁴ Nor have later historians addressed the awkward possibility that the captains of heavy industry and political leaders were moving in opposite directions, one toward interdependence and the other toward war. According to Fritz Fischer, Germany's leaders decided that war would not only solve the country's political difficulties but would also improve its long-run economic position. Germany's leaders, Fischer asserted, especially feared the shortage of iron ore, because the economic penetration of France had been set back by 1912 and Sweden since 1907 had put limits on the exports of ore.¹¹⁵ But it seems doubtful that Germany's militarist leaders could justify a European war on these economic grounds. Many of the iron ore concessions that German industrialists had obtained in France had not yet even begun to be mined on the eve of the war. Sweden, meanwhile, had put only a percentage cap on iron ore exports to Germany: since the volume of iron ore production and exports rose enormously after 1907, the 80 percent that went to Germany increased from 2.5 to nearly 5 million tons between 1909 and 1913.¹¹⁶ Indeed, Fischer's larger

¹¹³ Wolfgang Mommsen, "Nationalism, Imperialism, and Official Press Policy in Wilhelmine Germany 1850–1914," *Opinion publique et politique extérieure: Colloque*, Vol. 1: 1870–1915 (Rome, 1981); Philip G. Nord, *Paris Shopkeepers and the Politics of Resentment* (Princeton, N.J., 1986); Michael Tracy, *Agriculture in Western Europe* (New York, 1964), 34–35.

¹¹⁴ Norman Angell, *The Great Illusion* (London, 1913); Henry Noel Brailsford, *The War of Steel and Gold: A Study of the Armed Peace* (London, 1917). Contemporary opinion did focus on multinational iron ore concessions in North Africa and the manufacture of armor plate; Alphonse Merrheim, *L'Affaire de l'Ouenza* (Paris, 1910), and H. Robertson Murray, *Krupp and the International Armaments Ring* (London, 1915), 150–57.

¹¹⁵ Fischer, *War of Illusions*, 322–26.

¹¹⁶ Berthold Steinhoff, *Die schwedische Eisenerzproduktion und Eisenerzpolitik seit der Jahrhundertwende* (Berlin, 1937), 68. It had been difficult since 1900 for foreigners to purchase Swedish ore fields, but some continued to do so until foreign purchase was banned in 1916; Steinhoff, 88–93. Much of Fischer's evidence comes from Hermann Schumacher, "Die volks- und weltwirtschaftliche Bedeutung der Moselkanalisierung," *Technik und Wirtschaft*, 3 (1910): 705–25, which, though informative, is also a piece of special pleading, based on nationalism, for a Mosel canal that could better connect German Lorraine with the rest of Germany. Its pessimism about ore supplies was already questionable by 1913. Other writers were less pessimistic; Sehmer, *Die Eisenerzversorgung Europas*,

argument, that the military elite drove Germany into war, is stronger if one sees the elite as ignoring economic interests and convincing itself that a quick war of conquest would have economic, as well political and military, advantages.¹¹⁷ Nor is it clear that German industrialists' economic penetration of France and other countries before 1914 prefigured German annexationism during the war.¹¹⁸ From the French point of view, Belgian industrialists such as Gustave Trassenster, head of Ougrée-Marihay, acted little differently than did the Germans.¹¹⁹ Industrialists pursued their multinational ventures, while taking for granted the maintenance of peace and an open, European-wide market that made these ventures possible. They little realized that political leaders valued their countries' military and diplomatic position most of all and had scant regard for how the international economy functioned.

World War I began a thirty-year process of destroying almost all economic integration in heavy industry between Continental European states—except for that which depended on force. West European leaders still recognized the need to link the iron and coal sectors across countries, but they rejected genuine economic integration. Although political leaders usually took the lead in this rejection, big business did little to defend international cooperation. Yet almost all the substitutes for integration were profoundly flawed. During World War I, the Germans' plans for annexing Belgium and creating an autarkical *Mitteleuropa* ignored Continental heavy industry's dependence on exports: Germany had exported 35 percent, and Belgium 80 percent, of its steel.¹²⁰ As shipping magnate Albert Ballin remarked ruefully, "If *Mitteleuropa* should be our future, then we will have to force half our population to emigrate."¹²¹ French plans to displace Germany after the war hardly fared better. The project combining Luxembourg, Alsace-Lorraine, the Saar, and Belgium into a single coal and steel community with France floundered—and the Franco-Belgian attempt to add the Ruhr in 1923 did nothing to salvage it. Belgium and Luxembourg still needed to export, while France clung to protectionism, and there was almost no way to avoid trading French iron ore for German coal.¹²² The European and International Steel Cartels finally gave Continental heavy industry a precarious stability, but they

354–56, and Einecke and Kohler, "Die Eiservorräte." Fischer admits that Poidevin, the foremost authority on Franco-German economic relations, disagrees with his interpretation of a breakdown before the war; Fischer, *War of Illusions*, 325–26.

¹¹⁷ Fritz Fischer, *Germany's Aims in the First World War* (New York, 1967).

¹¹⁸ "The industrialists in general demanded from the Government the annexation of Longwy-Briey, and also that of Belgium or at least its economic domination by Germany. They thus pursued the same tactics in war as in peace"; Arthur Rosenberg, *Imperial Germany: The Birth of the German Republic, 1871–1918* (Boston, 1964), 101. See also Hans Gatzke, *Germany's Drive to the West* (Baltimore, Md., 1966), 11–35.

¹¹⁹ Rust, "Business and Politics," 158–62; Moine, *Les barons du fer*, 380, 422.

¹²⁰ Friedrich Naumann, *Central Europe* [1915] (London, 1917); Gatzke, *Germany's Drive*, 38–62, 105–71; Fischer, *Germany's Aims*, 108–11, 167, 591–98; Georges-Henri Soutou, *L'or et le sang: Les buts de guerre économiques de la Première Guerre mondiale* (Paris, 1989), 52–83; Henry Cord Meyer, *Mitteleuropa in German Thought and Action* (The Hague, 1955), 145–250.

¹²¹ Lamar Cecil, *Albert Ballin: Business and Politics in Imperial Germany, 1888–1918* (Princeton, N.J., 1967), 317.

¹²² Jacques Bariéty, *Les relations franco-allemandes après la Première-Guerre mondiale* (Paris, 1977), 121–92; Charles S. Maier, *Recasting Bourgeois Europe: Stabilization in France, Germany, and Italy in the Decade after World War I* (Princeton, N.J., 1975), 194–209, 272–304, 387–420.

were rarely more than truces between the various national cartels.¹²³ The lengths to which Germany went to obtain ore demonstrate the economic logic behind integration. During the Spanish Civil War, the Nazi regime forced Franco to shift iron ore exports from Britain to Germany, and, briefly, on the eve of World War II, Germany was dangerously dependent on Swedish ore.¹²⁴

Despite the continuing advantages of interdependence, during the interwar years both international business in heavy industry and labor migration contracted. The Germans never revived the holdings they lost in Belgium, Luxembourg, and Alsace-Lorraine, the surviving multinationals like Ougrée-Marihay nearly went bankrupt, and the French even ended up selling off some of the holdings they had acquired after the war in Luxembourg and the Saar.¹²⁵ Aside from some expansion in automobiles and food processing, multinational investment in Europe shrank in importance by comparison with investment in the rest of the world.¹²⁶ Although there were some large movements of immigrant workers during the interwar period, the relatively free movement of labor disappeared. Germany restricted still further the rights of foreign workers to enter that country.¹²⁷ By 1929, heavy industry in Luxembourg had increased its work force by half from the level of 1913, but native workers now filled 60 percent of the jobs as opposed to only 40 percent before the war.¹²⁸ France became more dependent than ever on immigrant workers, but immigration was now strictly controlled by nation-to-nation agreements—for example, between Mussolini's Italy and France.¹²⁹ During the Depression, France (and to a lesser extent, Belgium) pushed out immigrant workers as a way to export their unemployment.¹³⁰

After the failure of Hitler's attempt at European empire, the only hope for prosperity in Continental heavy industry was a return to at least a degree of economic interdependence. France, Luxembourg, Belgium, Germany, and the Netherlands—those countries that had been moving toward interdependence in iron and steel in the pre-World War I era—founded the European Coal and Steel Community. The sixth member country was Italy, which had been the major

¹²³ Ulrich Nocken, "Das Internationale Stahlkartell und die deutsch-französischen Beziehungen 1924–1932," *Konstellationen internationaler Politik 1924–1932*, Gustav Schmidt, ed. (Bochum, 1983); Daniel Barbezat, "Cooperation and Rivalry in the International Steel Cartel, 1926–1933," *Journal of Economic History*, 49 (1989): 435–47.

¹²⁴ Robert H. Whealey, *Hitler and Spain: The Nazi Role in the Spanish Civil War, 1936–1939* (Lexington, Ky., 1989), 72–94; R. Karlbom, "Sweden's Iron Ore Exports to Germany, 1933–1944," *Scandinavian Economic History Review*, 13 (1965): 65–73; Alan Milward, "Could Sweden Have Stopped the Second World War?" *Scandinavian Economic History Review*, 15 (1967): 127–38.

¹²⁵ Bariéty, *Les relations franco-allemandes*, 144–71; Fritz Pflug, "Die Internationale ARBED," *Deutsche Volkswirt* (August 23, 1940): 1719–22; Eric Bussière, "La sidérurgie belge durant l'entre deux-guerres: Le cas d'Ougrée-Marihay (1919–1939)," *Revue belge d'histoire contemporaine*, 15 (1984): 303–80.

¹²⁶ Dunning, "Changes in the Level and Structure," 85–93.

¹²⁷ Klaus J. Bade, "Labour, Migration, and the State," in Bade, ed., *Population, Labour, and Migration in 19th- and 20th-Century Germany* (New York, 1987), 81.

¹²⁸ Wagner, *La sidérurgie luxembourgeoise*, 189.

¹²⁹ Donald Reid, "The Politics of Immigrant Workers in Twentieth Century France," in Guérin-Gonzales and Strikwerda, *Politics of Immigrant Workers*, 245–78.

¹³⁰ Cross, *Immigrant Workers in Industrial France*, 203–07; Frank Caestecker, "Het Vreemdelingenbeleid in de Tussenoorlogse Periode, 1922–1939 in België," *Belgisch Tijdschrift voor Nieuwste Geschiedenis*, 15 (1984): 461–86.

supplier of immigrant labor.¹³¹ From a historical perspective, the Coal and Steel Community was a mixture of both the pre-1914 system and the interwar cartels. Prices and production were now strictly regulated, as though under cartels, but national cartels no longer had the power to struggle with each other behind the scenes. Prices and production levels were required to be public and under the scrutiny of an international institution.¹³² The only way to stabilize the system was to encourage the movement of coal and iron across borders, fulfilling the old function of the international firms. And, just as they had found, the Coal and Steel Community soon saw that the freer movement of labor was a necessary complement of this interdependence. The deep involvement of government in heavy industry to protect employment made it difficult to re-create multinationals in iron and steel.¹³³ Yet, once the European Community was built on the foundation of the Coal and Steel Community, multinational manufacturers who expanded across Europe in a variety of other industries were able to follow the path first taken before 1914.

The new international economic institutions were only possible because the position of key interest groups and the relationship between governments and big business had changed greatly with the rise of the welfare state and social democracy. One aspect of this reorientation of European politics was that farmers and the lower middle class now had less temptation to respond to economic nationalism as a symbol for their discontent with capitalism and industrialization.¹³⁴ The European Community's Common Agricultural Policy and the myriad of regulations protecting small commerce helped ensure that a new Pan-German League or Action Française would arise only with difficulty. The existence of the European Community and the acceptance of the notion of social welfare—despite all its failings—meant that international integration would be seen differently than it was before 1914. At that time, it had been a natural process brought about by private interests but not necessarily one in which any group had a stake beyond its own immediate material benefit. Beginning in the 1950s, there was an attempt to establish the notion that regulated integration could conceivably support the common good.

A NUMBER OF LESSONS CAN BE DRAWN from Europe's troubled history of integration. First, the history of multinational business in heavy industry suggests a rethinking of the debate on the origins of World War I. The alliances historians have found between conservative, agrarian, or militarist elites and heavy industry

¹³¹ Alan S. Milward, *The Reconstruction of Western Europe, 1945–51* (Berkeley, Calif., 1984), 126–67, 362–95; John Gillingham, *Coal, Steel, and the Rebirth of Europe, 1945–1955: The Germans and French from Ruhr Conflict to Economic Community* (Cambridge, 1991), 313–71.

¹³² Jean-Paul Courthéoux, "Les pouvoirs économiques et sociaux dans un secteur industriel: La sidérurgie," *Revue d'histoire économique et sociale*, 38 (1960): 364; Dennis Swann, *The Economics of the Common Market*, 6th edn. (London, 1988), 4–7, 85.

¹³³ J. E. S. Hayward, "Steel," in *Big Business and the State: Changing Relations in Western Europe*, Raymond Vernon, ed. (Cambridge, Mass., 1974). The supply of raw materials has also changed enormously; Edmond Dourille, *La sidérurgie dans le monde depuis 1952* (Paris, 1981), 103–10, 131–41.

¹³⁴ Rudy Koshar, ed., *Splintered Classes: Politics and the Lower Middle Classes in Interwar Europe* (New York, 1990); Heinrich A. Winkler, *Mittelstand, Demokratie und Nationalsozialismus* (Cologne, 1972).

should perhaps be reinvestigated. Big business before the war does not seem to have had a clearly nationalist program, or at least its motives were much more complex and tension-ridden than we have thought.

Second, it seems clear that the integration that has occurred since the 1950s, in a purely business or economic sense, could have happened much earlier. The industrial corporations of 1913 were as advanced in their ability to organize multinational production as firms would be in the 1960s. Schemes of gradually evolving multinational firms or stages of financial and industrial capitalism thus do not apply very well. Labor migration, too, was almost as free as anyone can imagine it today. Theories of economic integration and business history hence are untenable without a heavy dose of political economy.

Third, the length of time it took for internationalism to attract support points directly to how powerfully political decisions act on economic trends. Industrialists, labor leaders, and politicians at various times recognized the value of international ties, but only rarely did they see internationalism itself as a value or work to institutionalize international cooperation. Yet without some such political agreement to protect or encourage connections between states, it was easy for opponents to argue that internationalism was harmful. How could France possibly benefit from German investment? What advantages did Germany gain by making concessions to France in return for connections across borders? In this battle over public opinion, the malleability of business is sobering. Industrialists saw the economic rationality of internationalism, but they cooperated just the same with nationalist programs. For all the continuities in European heavy industry from 1914 to the Coal and Steel Community, the changes were profound. European integration only succeeded when very broad coalitions of farmers, business, labor, commerce, and government could be brokered into supporting it. Despite the theoretical perspectives to the contrary, integration is never inevitable. Without political coalitions to distribute the benefits of integration and to ameliorate its disadvantages, internationalism cannot succeed, regardless of the economic logic behind it.

Recognizing the precociousness of early economic integration could lead, finally, to reformulating our chronology of the twentieth century. World War I, in short, was a true break in time. And the 1914 to 1989 era may yet be seen as a difficult journey back to the historical route that Europe was taking decades earlier. Our history may someday be seen as moving in ellipses, in which the "shorter twentieth century" of 1914 to 1989 lies as a detour between the long nineteenth century and the twenty-first century that lies ahead.

Economic Integration and the European International
System in the Era of World War I

PAUL W. SCHROEDER

NOT BEING AN EXPERT ON THE ECONOMIC ASPECTS of Carl Strikwerda's interesting and provocative article, I intend to comment chiefly on how it affects our general picture of nineteenth and twentieth-century European international relations. It seems to me to have important implications in three respects. First, the analysis it gives of the course of European economic integration, which I take to be basically sound, clearly deals with a major element in the overall picture. Second, Strikwerda offers a suggestive interpretation of the actual and potential effects of this economic integration on the subsequent course of twentieth-century international history. My reasons for disagreeing with this interpretation in certain respects will be the main focus of these comments. Finally, his article exemplifies a general approach to international history and a conception of it that I consider valuable not only for historiography but also for public education and policy.

As already indicated, I can add little to Strikwerda's main argument on the extent to which West European industry, especially coal, iron, and steel, was integrated by 1914. His account, to be sure, seems to confirm rather than drastically alter the prevailing picture of a European economy largely open and fairly well integrated in some major sectors. One might view the debate¹ over whether nationalist protection or internationalist integration was the dominant economic trend by 1914 as one in which either side could be right, depending on how one judged not merely the factual evidence but also the eventual outcome. An additional comment might be that any implied or explicit argument that further economic integration in the early twentieth century might have averted a general European war would have to take more than Western Europe into account. The issues, tensions, and rivalries that actually produced the war, including economic ones, lay more in Central, Eastern, and Southeastern Europe. And when Strikwerda argues that "the creation of international business connections before World War I indicates the relative openness of the societies at the time," while the reaction against these ties and their failure to lead to durable

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¹ Summarized in Carl Strikwerda, "The Troubled Origins of European Economic Integration: International Iron and Steel and Labor Migration in the Era of World War I," *AHR*, 98 (October 1993): 1109-12.

cooperation indicates the strength of nationalism and the "failure of the European political system to reconcile the power of nation-states with the need for economic integration," few will disagree, but one might ask whether these are the only implications to be drawn or the most important ones.² Does not this also show how fragile that societal openness was, how shallow were the roots of integration and internationalism? Was the political failure to reconcile the power of nation-states with the need for economic integration perhaps a "failure" to do something impossible and almost inconceivable at the time?

Still, these are comments and questions, not criticisms. They do not detract from the strong case Strikwerda makes for the advanced state of the integration of key West European industries by 1914 and the persistence of this trend despite growing political, military, and nationalist rivalries. One can even accept that this trend offered possibilities for drawing nations together³ (although, as political scientists sometimes point out, interdependence can produce greater friction as well as greater harmony) and agree with the author that Europe in 1914, rather than being borne inexorably toward war on a wave of nationalist hostility, was actually balanced uneasily between diverse trends and possibilities, including those of economic integration and nationalism.

WHEN IT COMES TO THE INTERPRETATION OF THESE TWO TRENDS and their relationship to each other, however, I find myself in important disagreement with him. He sees them as polar opposites, representing two divergent paths Europe could have taken: economic integration leading potentially toward further international cooperation and peace, or "self-interested and destructive nationalism" leading toward protectionism, militarism, and war.⁴ Yet, while he notes that the forces promoting political and economic nationalism were in alliance, he recognizes correctly that there was no similar alliance between the industrialists who benefited from economic integration and those promoting the cause of international political cooperation. In fact, he writes, "German industrialists themselves undercut the logic of international economic integration" by an alliance with conservative military and nationalist forces against labor and the Socialists. This inconsistency, he argues, was the product of the industrialists' political immaturity (a failure to see their need for a political alliance in favor of economic internationalism) and of the immaturity of economic integration itself, which "had not gone far enough to force German industrialists to choose between national chauvinism and international cooperation."⁵ While industrialists, as he insists (again, I think, largely correctly), were not pushing for war or annexations before 1914, they failed to understand the root requirements for their own security and prosperity in the international arena or to be sufficiently concerned about them:

² Strikwerda, "Troubled Origins," 1111-12.

³ Strikwerda, "Troubled Origins," 1123.

⁴ Strikwerda, "Troubled Origins," 1123.

⁵ Strikwerda, "Troubled Origins," 1124.

Industrialists pursued their multinational ventures, while taking for granted the maintenance of peace and an open, European-wide market that made these ventures possible. They little realized that political leaders valued their countries' military and diplomatic position most of all and had scant regard for how the international economy functioned.⁶

In other words, Strikwerda sees economic integration and nationalism not merely as two divergent trends leading toward two sharply different outcomes but also as representing two opposed ways of thinking, two divergent collective mentalities, each with its own set of blinders. The industrialist economic-integration mode of thinking called for international economic and political cooperation but failed to understand that this kind of cooperation would require a different set of rules for the conduct of international politics generally. The political-military-nationalist mode of thought operated on traditional assumptions of international rivalry, coercion, and conflict and failed to understand or care about what this would do to an increasingly integrated international economic system.

Leaving certain broader questions about the general character and sources of pre-1914 nationalism aside,⁷ this dichotomy does not seem to me the best way to understand either the objective interrelationship between international politics and economics in pre-1914 Europe or the way most European military, political, and economic leaders thought about it. The problem and the main source of the tragedy was not that leaders from different camps offered two divergent programs for achieving national security and prosperity, economic integration versus nationalism, each involving mutually incompatible ideas and assumptions. It was rather that leaders from all camps reached substantial agreement on one program, differing only on details of emphasis and execution. By and large, everyone assumed that the state, to ensure its citizens' security and prosperity, had to succeed simultaneously in two fields of competition in the international arena, both inextricably intertwined and mutually interdependent: politics and economics. The political and military strength and security of the state increasingly required a strong, modern domestic economy to support it; a strong domestic economy in turn increasingly required the state to develop and use adequate diplomatic and military resources to ensure that its economic activity would enjoy favorable or at least tolerable conditions in the world marketplace.

Naturally, there were important disagreements over just how to make this mutual interdependence and support between international politics and economics work—disputes over protectionism versus free trade, autarky versus economic integration and interdependence, security through armaments and military alliances versus security through Concert diplomacy and multilateral agreements. Of course, particular interests, ideologies, and outlooks played a powerful role in these disputes. Yet no influential groups challenged the need for governments and economic sectors to work closely together to guarantee the nation's security.

⁶ Strikwerda, "Troubled Origins," 1126.

⁷ Considerable scholarship on pre-war Germany, for example, pictures nationalism as radical rather than conservative in character and arising from the middle class rather than being manipulated from above by governmental, military, or economic leaders. See David Blackbourn, *Class, Religion, and Local Politics in Wilhelmine Germany* (New Haven, Conn., 1980); Geoff Eley, *Reshaping the German Right* (New Haven, 1980); and Blackbourn and Eley, *The Peculiarities of German History* (Oxford, 1984).

In other words, no major groups in any state really abandoned nationalism in favor of international integration, either in politics or in economics. Even those interested in international economic integration wanted the national state to support them in it, to help ensure their safety and success. All the great trends and lessons of late nineteenth and early twentieth-century history reinforced this outlook. The lesson of German and, to a lesser extent, Italian unification was that national unity promoted success in the world economy, that it made commerce, both competitive and cooperative, more possible and beneficial. Imperialism, the great scramble for trade, colonies, and spheres of influence in the extra-European world, pressed the same point home. The real question was not whether Europe could best exploit its imperialist opportunities by international cooperation or by nationalist competition possibly leading to war. Instead, the reigning (and correct) assumption was that both cooperation and competition were possible and necessary in various circumstances but always on a fundamentally nationalist basis, between states striving to protect their national interests, political and economic. This seemed to work. Compared to the sixteenth, seventeenth, and eighteenth centuries, nineteenth and twentieth-century European overseas imperialism was remarkably peaceful, at least for the metropolises. The colonial scramble never led to war between European powers, and seldom seriously threatened to. Most issues were settled by mutual agreement, shares of the loot divided up more or less amicably, trade permitted with each other's colonies (the largest empire, the British, remained a free trade empire), and joint economic enterprises entered into. Nonetheless, everyone knew this to be an intensely competitive arena in which the government and various economic interests had to work together for success and in which states with governments and economies too weak, ill-organized, or distracted to do so (Italy, Austria-Hungary, Spain, Portugal) usually lost out.⁸

Even World War I did not destroy but confirmed the belief that to participate successfully in world politics and the world economy, including its vital aspects of international economic integration and cooperation, one had to have a strong state uniting and coordinating political, diplomatic, military, and economic strategies. Strikwerda's verdict that the war had a disastrous impact on international economic integration, and that postwar attempts to revive it were flawed, once again seems basically sound. Yet, as recent studies have shown, the pre-war liberal international order demonstrated a remarkable attractiveness and resiliency even during this cataclysmic conflict.⁹ The various powers, including imperial Germany, rather than simply abandoning an international order once war began in favor of all-out protectionism or autarky, instead in various ways sought through military victory to maintain it, only changing it to ensure that they

⁸ For an example of how this worked, see F. R. Bridge, "Tarde venientibus ossa [the latecomers get the bones]: Austro-Hungarian Colonial Aspirations in Asia Minor 1913–14," *Middle Eastern Studies*, 6 (1970): 319–30.

⁹ Especially Georges-Henri Soutou, *L'or et le sang: Les buts de guerre économiques de la Première Guerre mondiale* (Paris, 1989); Horst Günther Linke, *Das zarische Russland und der erste Weltkrieg: Diplomatie und Kriegsziele, 1914–1917* (Munich, 1982); and Bernd Bonwetsch, *Kriegsallianz und Wirtschaftsinteressen: Russland in den Wirtschaftsplänen Englands und Frankreichs, 1914–1917* (Düsseldorf, 1973).

controlled it and enjoyed its advantages while their defeated foes bore its burdens—a goal not fundamentally different from the aim pursued in peacetime.

In other words, Strikwerda is right in perceiving a disjunction and ultimate contradiction between the world being gradually created by the trend toward economic integration before 1914 and the actual world of pre-war international politics and economics, governed by a competitive nationalist ethos and set of rules. He is wrong, however, in supposing that this disjunction could have been widely or clearly perceived before 1914 or that, had it been perceived, political and economic leaders could have done much to make the old international rules conform to the new emerging economic realities. The prevailing concept of how the international system worked, with political, military, and economic competition between national units forming part of a single interwoven complex, each strand within each national unit reinforcing the others, was too deep-rooted and universal. Strikwerda seems to imply that, had economic integration continued to develop without being derailed by nationalist pressures and war, Europe would eventually have insensibly reached the point where (as now in Western Europe) war between its various states would become a practical impossibility, too destructive of too many ties and interests to be seriously contemplated. Europe, in short, could have grown out of war through the process of economic integration, with *realpolitik* gradually giving way to international cooperation. Not only do the prevailing historical conditions and mindsets argue against any such hypothesis, one must add the consideration that no state or people involved in a high-stakes, fiercely competitive contest, even if it is losing badly, can risk dropping out and switching to a game based on cooperative rules without firm assurances either that all the other players will also simultaneously switch from conflict to cooperation or, failing that, that some other player will protect its interests in the current contest. Without those assurances, any such action simply makes one a sacrificial lamb.

The disaster in 1914 did not derive therefore from a failure by industrialists to understand the political logic and requirements of economic integration or even the failure or refusal of politicians, military men, various interest groups, and broad publics to appreciate the long-range advantages of peaceful international cooperation over unrestrained competition and conflict. It lay rather in the structure of international politics—the fact that its component individual states would not and could not, either separately or together, leap from a power-based competitive international system to a rule-based cooperative one. For governments and peoples effectively to realize that an international system dominated by power-political competition is in the long run incompatible with real, durable international economic integration and its benefits, and for them genuinely to opt for the latter rather than merely wish for it, they must first be convinced that the power-political game has become intolerably expensive and dangerous and must be abandoned and also persuaded that another more cooperative system is available, or at least possible, and that the other important players will try it as well or, if not, that some other player or players will protect them and their interests if they alone defect from the competition. None of these essential conditions prevailed before 1914.

All this affects Strikwerda's interesting argument about periodization. His suggestion that the West European economic integration launched after World War II under state direction represented a kind of reprise, a renewal of a long nineteenth-century process of economic integration interrupted by 1914, again seems to me broadly correct. Yet if real, durable economic integration logically requires first a discrediting and collective repudiation of power politics as the primary basis for international politics and economics alike, this affects how we periodize the nineteenth century (1815–1914) and the twentieth (1914–1989). Both need to be broken into smaller units. The early nineteenth century, lasting from 1815 to the early 1850s, was a period in which power politics was actually discredited and dethroned as the primary basis of European international relations. This same period also laid the foundations for early economic liberalism and international integration in much of Europe (Britain, Prussia, Germany, Belgium), which by the 1840s was beginning to spread elsewhere (France, northern Italy, even Austria). The last half of the nineteenth century, however, was dominated by two divergent trends in international relations: on the one hand, a rapid if uneven economic expansion and some real but limited international economic integration and interdependence; on the other, the revival and reaffirmation of power politics as the basis for international relations and the primary, almost exclusive, method of ensuring national security and prosperity.¹⁰ Strikwerda's essay shows, among other things, how these two principles ultimately collided. The period 1914–1945 represents a more terrible reprise of the revolutionary and Napoleonic wars, in which once more, this time for good, both power politics and economic autarky were discredited as the bases for international life, at least in Europe. The period 1945–1989 represents in Western Europe a revival of the principles of 1815 on a far broader, more durable basis—the dethroning of power politics, the recognition of the ultimate futility and destructiveness of traditional “self-help” methods, and a deliberate movement toward political and economic integration and interdependence as not merely a better way but the only way to achieve security and prosperity.

I DO NOT WISH TO DWELL ON THESE DIFFERENCES of interpretation, however, or press my scheme of periodization, and still less to become mired in a sterile debate over which comes first and is more important in the transformation of international relations, politics or economics, the repudiation of power politics or the

¹⁰ There is a striking historical irony here. The post-1815 period is regularly called the Age of Restoration and the period from 1848 to 1870 the Age of Liberalism and Nationalism. Yet the post-Vienna period, as I argue in a forthcoming book, was restorationist only to a limited degree even in domestic politics and not at all in international politics, where it produced much progressive change. As for 1848–1870, it undoubtedly brought a rise in liberalism and nationalism and some major instances of national unification. Yet, so far as international politics is concerned, this was clearly the Age of Restoration—a return to the reign of power politics. In domestic politics and economics, its greatest statesmen, Palmerston, Cavour, and Bismarck, were all more or less liberal or progressive; in international politics, however, they all consciously returned to eighteenth-century *realpolitik* as the norm for international conduct. See Paul Schroeder, *The Transformation of European Politics, 1763–1848* (Oxford, 1994), vii–ix, 575–82, and *passim*.

achievement of economic integration.¹¹ Far more important than any differences on these scores or others are the aspects of Strikwerda's picture of international relations on which we agree. His article clearly portrays it not as an arena of structurally determined competition and conflict but a sphere of human activity like many others, constantly changing and subject to transformation along with other aspects of society, especially as the result of economic developments. Still more important, it is a sphere not merely of change and development but also of collective learning. Peoples and states can and do learn over time how to order their international relations better, though with great difficulty and usually only under extreme pressure. They can and do adapt to new realities, find different and better ways to achieve their goals. This means that past failures, however tragic their consequences, are not simply failures but can and should be instructive preludes to new efforts.

These may sound like tedious bromides, Pollyannish sentiments. In fact, they have great current relevance. The most startling and important thing that has happened in world international relations since 1985 is not, in my view, the end to the Cold War, the disappearance of the Iron Curtain, the collapse of Communism, the unification of Germany, or even the dissolution of the Soviet Union. Dramatic, unexpected, and crucial though all these are, they have historical analogues and precedents. One great change does not. For the first time, a number of important states, including one superpower, territorially the largest and militarily the second most powerful state in the world, the USSR, chose to repudiate power politics and the balance of power as the prime basis for their security and prosperity, recognizing its counterproductive effects and ultimately intolerable consequences in the modern world, and to rely instead on achieving cooperative relationships with formerly bitter adversaries. In addition, they did this more or less voluntarily, without having experienced exhaustion or catastrophic defeat in a total war, as in 1815 and 1945.

Nothing could be a more dramatic proof of the possibilities for constructive change and learning in international politics than this development. Nothing is more vital for world peace and security than that this unique, courageous experiment be supported and carried through to the point of ultimate success. Every other desired end in Europe and the rest of the world—economic integration, the expansion of trade, the growth of liberal democracy, the protection of civil rights, the prevention of new ethnic and international conflict—depends to some degree on sustaining it, avoiding a reversion to power politics. What has happened and is happening in the former Yugoslavia is only a small reminder of the dangers of such a retrogression.

Yet within the American foreign policy community, and doubtless elsewhere, a vocal, influential group is proclaiming the message that none of this really happened; that the old power politics is not at all obsolete and has not been repudiated or that, if it has been, it was all a big mistake, and the sooner we

¹¹ If forced to state a view, I would say that power politics has to be dethroned and effectively curbed, though not wholly abolished, in order for any other changes to be secure, but that once this is done, both international political reform and economic integration can proceed side by side, and that of the two, economic integration is probably the more powerful agent in transforming international relations.

all—Americans, Russians, Germans, Japanese, West Europeans, Ukrainians, everybody—return to relying for ultimate security and prosperity on military strength and alliances, global and regional balances, mutual nuclear deterrence, and the other proved and solid devices of power politics, the better and safer we all shall be.¹²

This is not the place to argue against this (to me) recklessly unhistorical view. It may not be inappropriate, however, to use a little space in the official journal of the American Historical Association to plead for greater participation by historians, especially international historians, in vital current debates such as this. International politics is too important to be left solely to political scientists and journalists. The public and policy makers need to learn from us, through articles like Strikwerda's and in other ways, that there is a long view on international politics and that it is one of change, development, and learning; that we are not imprisoned in the fatal patterns of the past but can do better.

¹² The leading school in this kind of thinking (though by no means all members share it) is that of neorealism or structural realism, best expounded in Kenneth N. Waltz, *Theory of International Politics* (Reading, Mass., 1979), which sees power politics as at all times the central, structural determinant of international relations. For specific examples of the arguments I allude to, see John J. Mearsheimer, "Back to the Future: Instability in Europe after the Cold War," *International Security*, 15 (1990): 5–55; Christopher Layne, "The Unipolar Illusion: Why New Great Powers Will Rise," *International Security*, 17 (1993): 5–51; Mearsheimer, "The Case for a Ukrainian Nuclear Deterrent," *Foreign Affairs*, 72 (Summer 1993): 50–66.

Response to "Economic Integration and the European
International System in the Era of World War I"

CARL STRIKWERDA

IN HIS THOUGHTFUL COMMENT, PAUL SCHROEDER has pointed out that the debate over economic integration and the origins of World War I turns on one of the central questions of modern politics: the relationship between political and economic power. And, as he correctly points out, the way in which leaders and the public *believe* political and economic power are related is as important as how these two kinds of power actually do interact. Given the present momentous crossroads in the post-Cold War world, I can only second Schroeder's call for policy makers and the public to work for mutually beneficial economic and political relations between nations. In addition to the need to reach out to the former Soviet bloc, which Schroeder justly stresses, one could also argue that U.S.-Japanese relations, the North American Free Trade Agreement, and European integration are areas in which we need to remind ourselves that, in the long run, the advantages of cooperation and negotiation outweigh the dangers inherent in conflict and self-interested nationalism.

Although Schroeder's comment provides a number of important insights, I wonder whether we can perhaps draw a stronger lesson than he does from history. Schroeder argues that, despite the international cooperation in the economic sphere before 1914, a major war was still virtually inevitable. Leaders and most of the public believed, first, that international economic relations, just like great power politics, were necessarily rooted in conflict rather than cooperation and, second, that as a result states had to use political and military power to strengthen their international economic position. "By and large, everyone assumed that the state, to ensure its citizens' security and prosperity, had to succeed simultaneously in two fields of competition in the international arena, both inextricably intertwined and mutually interdependent: politics and economics."¹

Yet, given that an enormous degree of economic integration did occur, perhaps we should also investigate whether all European leaders actually believed that political and military power were essential to economic success. Just because the assumption that international relations will always tend toward conflict has been so deeply rooted and because the results of that assumption can be so disastrous, historians would do well to question it. After all, if excellent scholars on the

¹ Paul W. Schroeder, "Economic Integration and the European International System in the Era of World War I," *AHR*, 98 (October 1993): 1132.

pre-war era like Sidney Pollard and Norman Stone could simply overlook economic integration, so the beliefs about political and economic power could also have been interpreted selectively.² Government action was often necessary to make economies more open and cooperative, and defense against a real military threat was a necessary consideration.³ But these kinds of state action were quite different from militarism or imperialism. To put it another way, the existence of the various "means" that Schroeder sees as the only way in which Europeans differed on advancing their countries' power might in fact bring very different ends. "Free trade," "economic integration," and "Concert diplomacy" led in a direction different from "protectionism," "autarky," and "armaments and military alliances."⁴ As Schroeder himself has suggested in an innovative article, by introducing consultative diplomacy, conservatives in the first half of the nineteenth century radically changed traditional great power politics.⁵ Liberals then introduced international economic cooperation in the mid-nineteenth century, after which Bismarck's wars reasserted militarism.⁶

It is possible, then, that, rather than a consensus, leaders had at their disposal a set of conflicting interpretations on whether political and military power helped a state economically. Imperialist propaganda occupies such a huge place in historiography—in part, because of its sheer volume—that we may be misled. Perhaps its volume testifies to the need to convince the public and policy makers of colonialism's value when economic trends moved in other directions. Britain's wealth did not depend on imperialism.⁷ Nor was it true that "the flag follows investment," or Britain would have tried to conquer Colorado.⁸ France owed more to its trade and flows of labor and capital with tiny Belgium than it did to its empire.⁹ In any case, as Schroeder reminds us, imperialism did not lead directly to war, since disputes were settled cooperatively.

We can also too easily read the pre-1914 era through the violence and polarization created by the world wars. For example, Germany's penetration of foreign economies before the war has frequently been linked to annexationism during the war. In what became one of modern European historiography's most-used quotations, the industrialist Hugo Stinnes declared in 1911, "three to four years of peace, and I can guarantee silent German dominance in Europe." But, as Hans Jaeger points out, Stinnes actually said this to Heinrich Class, leader of the Pan-Germans, to define the limits of the industrialists' support for

² Carl Strikwerda, "The Troubled Origins of European Economic Integration: International Iron and Steel and Labor Migration in the Era of World War I," *AHR*, 98 (October 1993): 1109–10.

³ Charles Kindleberger, "The Rise of Free Trade in Western Europe 1820–1875," *Journal of Economic History*, 35 (1975): 20–55.

⁴ Schroeder, "Economic Integration," 1132.

⁵ Paul Schroeder, "Did the Vienna Settlement Rest on a Balance of Power?" *AHR*, 97 (June 1992): 683–706.

⁶ Schroeder, "Economic Integration," 1135, and especially note 10.

⁷ Instead, Britain's wealth allowed it to have an empire. India was the one important example of a colony that paid. Lance E. Davis and Robert A. Huttenback, *Mammon and the Pursuit of Empire: The Political Economy of British Imperialism, 1860–1912* (Cambridge, 1986); Patrick O'Brien, "The Costs and Benefits of British Imperialism 1846–1914," *Past and Present*, 120 (1988): 163–99.

⁸ Henry Noel Brailsford, *The War of Steel and Gold: A Study of the Armed Peace* (London, 1917), 78.

⁹ Robert Frank, "L'Allemagne dans le commerce français," in *France and Germany in an Age of Crisis, 1900–1960*, Haim Shamir, ed. (Leiden, 1990), 32; Raoul Blanchard, *La Flandre: Etude géographique de la plaine flamande en France, Belgique, et Hollande* (Dunkirk, 1906), 395–459.

aggressive nationalism. Stinnes saw no need for war since German industry was doing so well.¹⁰

In fact, one of the best examples of the confusion over the value of military power for economic success is the inability of Germany's leaders to explain what they hoped to gain from launching the most devastating war in modern history. As industrialists and economic advisers warned again and again during the war, unless the pre-war international economy was revived, even extravagant territorial gains in Continental Europe might leave Germany worse off economically.¹¹ Before the war, Britain and other trading partners had not been shutting out German goods, but they probably would have if Germany had tried to set up a Continental empire. Similarly, when the Prussian parliament discussed Germany annexing iron ore fields, the Socialist member Otto Hué reminded the government that German and French industrialists had simply cooperated in owning and shipping ore before the war. The only response was that the war itself had rendered cooperation no longer possible and necessitated annexations.¹² The confusion among the elite about how Germany could improve its economic position over the pre-war situation exemplifies how much more research we need to do on the relationship between economics and politics in European history.

It is true that Eastern Europe—relations between Germany, Austria-Hungary, Russia, and the Balkan states—provided the spark for the war itself, and the argument has been made that economic integration here had less effect than elsewhere. The enormous migration of labor, among other factors, however, was drawing these economies together.¹³ The factors at work were similar. What we need to understand is why, just as in Western Europe, the political decisions to seek a supposedly stronger military position were made with so little reference to any real gains and with so little recourse to other solutions.

In many ways, this is a question for intellectual history or the social history of ideas as much as political history. Why did political leaders tend to believe in the end that international economic power needed to rest on political and military intervention? Schroeder suggests that players in a power-political game such as the pre-1914 world cannot change unless they believe "that another more cooperative system is available, or at least possible, and that the other important

¹⁰ Heinrich Class, *Wider den Strom: Vom Werden und Wachsen der nationalen Opposition im alten Reich* (Leipzig, 1932), 217, my translation; Hans Jaeger, *Unternehmer in der deutschen Politik (1890–1918)* (Bonn, 1967), 137. Unfortunately, this is also one of the more easily misused quotations. Stinnes was actually summarizing his earlier statement: "Let us have three to four years of peaceful development, and Germany will be the undisputed economic master of Europe." Gatzke uses the summary, which drops the reference to "peaceful development" and makes it unclear whether the dominance is political or economic. Hans Gatzke, *Germany's Drive to the West* (Baltimore, Md., 1966), 34. Gillingham has Stinnes say this to "his former classmates," but the source is Gatzke; John Gillingham, *Coal, Steel, and the Rebirth of Europe, 1945–1955: The Germans and French from Ruhr Conflict to Economic Community* (Cambridge, 1991), 11.

¹¹ Egmont Zechlin, "Cabinet versus Economic Warfare in Germany," in *The Origins of the First World War*, H. W. Koch, ed., 2d edn. (London, 1984); Strikwerda, "Troubled Origins," 1126, note 120.

¹² Gatzke, *Germany's Drive*, 170–71. Gatzke accepts the annexationists' argument that German firms had had difficulties obtaining ore from France before the war, 245–47; I see little evidence for this.

¹³ Lars Olsson, "Labor Migration as a Background to the First World War," paper delivered at the Eighth International Conference of Europeanists, Chicago, March 1992. Olsson argues, however, that the scramble for labor also increased tensions between Germany and its neighbors.

players will try it as well.”¹⁴ It is perhaps worth asking why leaders did not understand their own world as having the potential for this alternative, a cooperative system. It is surprising how few references there are in historical scholarship to the complicated web of international agreements that undergirded a world economy stretching from Australia to Russia. The International Postal Union, international telegraphic conventions, World Standard Time, the International Red Cross, and a host of other institutions connected states. Nor were these merely consultative; many had a direct impact on daily life and routine governmental activity. By one estimate, there were 119 new international organizations founded between 1900 and 1909 and an astonishing 112 more created between 1909 and 1914.¹⁵ What is more, leaders of all states—including the insecure nationalists of Germany, Russia, and the new states of the Balkans—wanted their countries to participate in these agreements. One other aspect of pre-World War I Europe illuminates how much the nation-states of the era coexisted and even assumed a foundation of international cooperation. As surprising as it may seem, there were virtually no passports, and visa regulations were nonexistent or, by later twentieth-century standards, incredibly lax. Thousands of businesspeople and workers migrated or went to work temporarily in other countries without encountering governmental controls.¹⁶ The pre-1914 era may suggest that international relations have not been as conflict-ridden as they appear and that cooperation has a basis in history just as the long history of human conflict does.

I agree with Schroeder that international relations are determined in a complex fashion by both changing social trends and shifting human activity. The critical question is why those who held power in Europe interpreted their world as they did. There is nothing inevitable about integration: it is a product of political decisions to allow economies to grow in that direction. For all their power in their own sphere, business leaders more often than not adapt to the political framework in which they find themselves, or, at most, they are one among a variety of interest groups trying to direct the way a political system changes. Political leaders, too, must choose to support integration by ameliorating its inevitable harmful effects. Just as integration is a product of political decisions, so the effect of economic integration on the political system depends on the lessons leaders and the public draw from it. Economic integration by itself cannot force political cooperation, although, as I argued, I believe that if it proceeds far enough, it can force the economic leaders carrying out integration to decide whether or not they wish to become advocates of international political cooperation as well. The German industrialists in 1914, I believe, were close to being faced with such a decision.¹⁷

¹⁴ Schroeder, “Economic Integration,” 1134.

¹⁵ John McManners, *Lectures on European History, 1789–1914: Men, Machines and Freedom* (Oxford, 1966), 361–62; Stephen Kern, *The Culture of Time and Space 1880–1918* (Cambridge, Mass., 1983), 11–16, 230–31. These references are not representative of contemporary historiography: McManners’s work is a textbook; Kern’s study has had great impact as a piece of intellectual history; but the political import of his argument has not been widely recognized by historians.

¹⁶ Kern, *Culture of Time and Space*, 194.

¹⁷ Strikwerda, “Troubled Origins,” 1124.

Crises such as those encountered by Europe in the years before 1914 and by the Western world in the collapse of the Soviet bloc are, as Schroeder suggests, too important not to be seen in the long view as both political and economic turning points. Diplomatic historians and social and economic historians have too often pursued their research in isolation from each other. Even when this reflects the frequent disjuncture in society between political systems and social trends, that very disjuncture, I would argue, raises important and intriguing questions. We need all the insights and all the re-thinking of old questions scholars can give in order both to learn from history and to face the challenges of today.

Review Article
Autocracy Tempered by Reform or by Regicide?

MARC RAEFF

Carol S. Leonard, **Reform and Regicide: The Reign of Peter III of Russia** (Bloomington: Indiana University Press, 1993), 232 pp.

John T. Alexander, **Catherine the Great: Life and Legend** (New York: Oxford University Press, 1989), 418 pp.

Roderick E. McGrew, **Paul I of Russia, 1754–1801** (Oxford: Clarendon Press, 1992), 405 pp.

IN AN AUTOCRACY, SURELY, THE AUTOCRAT PLAYS A DOMINANT ROLE, even when he or she is not alone in shaping and carrying out policies. Such was the case throughout the history of imperial Russia, from Peter the Great to the 1917 Revolution. Naturally, the autocrat has to be seen within the context of political institutions, government practices, and socioeconomic circumstances as well as the cultural environment; yet individual circumstances and individualities cannot be ignored either. It is, however, a curious historiographic fact that we have few scholarly studies of the occupants of the Russian throne. And, for this reason, the public's image of Russian emperors and empresses has been shaped by sensation-mongering, popular, and semi-fictionalized "biographies."

Until very recently, we have had only the massive, outdated (albeit replete with original archival documents) biographies of Peter the Great (by N. Ustrialov), of Paul I, and of Alexander I and members of their families (by N. Shil'der, E. Shumigorskii, and Grand Duke Nikolai Mikhailovich). These works offer much raw biographical data but do not relate them to their institutional, cultural, and socioeconomic contexts. Since Marxism plays down or denies the importance of the individual in history, Soviet historiography has all but ignored the part played by individual monarchs in the destinies of the Russian empire. Only two Western historians have written magisterial studies of the life and reign of the two extraordinary personalities who ruled Russia in the eighteenth century: Reinhold Wittram of Peter the Great and Isabel de Madariaga of Catherine II.¹

My references in this essay to related books and articles are not meant to be exhaustive but merely illustrative of the state of present-day historiography on the subjects under consideration.

¹ Reinhard Wittram, *Peter I: Czar und Kaiser*, 2 vols. (Göttingen, 1964); Isabel de Madariaga, *Russia in the Age of Catherine the Great* (New Haven, Conn., 1981). Over twenty years ago, I tried to give a "state of the art" report on the reign of Catherine II in: "Random Notes on the Reign of Catherine

In the last few years, however, in the wake of the decline and collapse of the Soviet system, interest in biographies of the Romanovs and of their helpmates has been rekindled in Russia—for example, the life and reign of Peter the Great and of the “fledglings of his nest.”² Due attention has also been paid to the character and role of the ruler by, for example, E. Anisimov on the period of Elizabeth, M. Safonov and S. Mironenko on Alexander I,³ to mention only those that have come to my attention. For their part, Western historians of Russia have also come to stress the biographical aspects in political history.⁴ As chance would have it, in the last few years there appeared three books focusing attention on the individual personalities of three monarchs who, together, reigned over the entire second half of the eighteenth century: Peter III (1761–1762), his wife Catherine II (1762–1796), and her son, Paul I (1796–1801). They offer an opportunity to reevaluate our understanding of the personal role played by the three monarchs. Let us, first, turn to an examination of the biographical dimension of each of the three studies.

Carol S. Leonard's *Reform and Regicide* is the first full monographic treatment of Peter III and his reign, and a major purpose of her book is to “rehabilitate” his reputation as both a person and a ruler. As practically all general histories and textbooks have it, Peter III was a mentally and emotionally “retarded,” ludicrous individual, enamored with all things Prussian, who scorned Russian national interest, derided its religion and culture, provoked the enmity of his wife and courtiers by his uncouth and shocking behavior, and in this way brought upon himself—after a six months' long reign—the coup that dethroned him and ended in his murder. Not so, argues Leonard, for the historians have relied uncritically on the memoirs of his wife, Catherine, who dethroned him, and her sycophantic admirers at home and abroad. Leonard shows convincingly that the anecdotes purporting to characterize Grand Duke Peter Fedorovich and Emperor Peter III are either not documented or based on a prejudiced and incomplete reading of the evidence. Far from being the ridiculous, almost retarded, brutal adolescent and young man depicted by Catherine II in her self-serving memoirs, Peter was in fact intelligent and educated, although perhaps not well disciplined and not as socially adept as was expected in Russian court circles. This was the unfortunate result of the faulty upbringing he received in the unhealthy atmosphere at the court of his aunt, Empress Elizabeth.

Leonard carries conviction with respect to the “negative” job she does in

II in the Light of Recent Literature,” *Jahrbücher für Geschichte Osteuropas*, n.s., 19, Heft 4 (December 1971): 541–56.

² N. Pavlenko, *Petr Pervyi* (Moscow, 1975); N. Pavlenko, *Pentsy gnezda Petrova* (Moscow, 1984); N. Pavlenko, *Petr Velikii* (Moscow, 1990).

³ E. V. Anisimov, *Rossii v seredine XVIII veka: Bor'ba za nasledie Petra* (Moscow, 1986); M. M. Safonov, *Problema reform v pravitel'svennoi politike v Rossii na rubezhe XVIII i XIX vv.* (Leningrad, 1988); S. V. Mironenko, *Samoderzhavie i reformy: Politicheskai bor'ba v Rossii v nachale XIX v.* (Moscow, 1989); and recently the vignettes of rules in issues of *Voprosy istorii*, nos. 4–5, 6–7 (1992).

⁴ True, mostly in connection with the nineteenth-century rulers: W. Bruce Lincoln, *Nicholas I: Emperor and Autocrat of All the Russias* (London, 1978); Andrew M. Verner, *The Crisis of Russian Autocracy: Nicholas II and the 1905 Revolution* (Princeton, N.J., 1990). In the Soviet Union, P. A. Zaionchkovskii pioneered in this respect with his *Rossiiskoe samoderzhavie v kontse XIX stoletia (Politicheskai reaktsiia 80-kh-nachala 90-kh godov)* (Moscow, 1970).

disproving the reliability of the literature on Peter III. She is, however, much less successful, it seems to me, in her "positive" endeavor at showing the grand duke and emperor as well versed in the literature and values of the Enlightenment and as having a comprehensive and coherent philosophy of government, a philosophy informed by progressive and reform-oriented goals in a physiocratic vein. True enough, we know from Jacob von Stählin, the tutor of Grand Duke Peter Fedorovich, that Peter did read books on cameralist science and jurisprudence. But we have no evidence of their impact on the mind of the heir to Elizabeth. In support of Leonard's view that Peter had physiocratic objectives and reform plans, she points to his legislation and to the fact that his main adviser on domestic policy, D. V. Volkov, had such objectives. But is this adequate proof of Peter having personally and consciously developed these reform plans? We shall return below to the specific policy measures of his brief reign. Leonard is most probably right in rejecting the traditional portrait of Peter III, but she does not adequately prove either his enlightened reformism or his personal involvement in shaping specific legislative acts. Concerning the question of the emperor's personal input into the various acts of his government, Leonard offers only circumstantial evidence—for example, his presence at council meetings or the fact that the final terms of an act were different from the drafts submitted after deliberation in government offices and on the recommendation of some councilor. Personally, I do not think that this focus is of primary importance, for it leaves open the question of the reasons for his overthrow by a coup that clearly did not stem from his policies.

John T. Alexander's biography of Catherine II, *Catherine the Great*, was published on the heels of Isabel de Madariaga's magisterial study of the empress's reign—and preceded but by little her concise, elegantly written shorter "digest" for a wide audience.⁵ In contrast to de Madariaga, Alexander deliberately focuses on the biographical, more particularly the private, side of his subject. Catherine's reign was not only very long (thirty-four years), in contrast to those of her husband and son, but it was also most seminal for the history of Russia. Alexander does chronicle the political events of the reign, yet he all but eschews offering analyses and explanations for their causes and long-range effects. True enough, the personality of Catherine was a crucial element in the shaping of her reign, and no doubt her private life as well was of significance. In the case of Catherine, the private was, apparently, the parade of her lovers. Catherine's love life has been the object of much prurient scandal mongering—largely by European pamphleteers aiming at discrediting her foreign policy (in particular, the partitions of Poland). Their alleged revelations have been the mainstay of most of the popular, semi (or totally) fictional "biographies" and movies dealing with Catherine II.

A biography and history of a significant ruler, which surely Catherine II was, needs to deal with the private life to the extent that it accounts for or explains the reign's policies and events. Alexander, however, indulges in extensive discussion of Catherine's amours and sex life, not so much to explain her public acts and role as to satisfy the prurient curiosity of adolescent college students (and the epilogue

⁵ Isabel de Madariaga, *Catherine the Great: A Short History* (New Haven, Conn., 1990).

is frankly in bad taste). This is unfortunate. The private behavior of the empress had a public, symbolic import in the "moral economy" of Russia's autocracy. Catherine was quite conscious of this side of her role and carefully kept her lovers out of public affairs. But as a woman in a man's world, she needed a "consort," albeit an unofficial one. Three men—G. Orlov, G. Potemkin, and P. Zubov—helped her carry the burden of government; they were not so much her paramours as her helpmates in the way Emperor Leopold was for Maria Theresa or—in the nineteenth century—Prince Albert for Queen Victoria. It is a pity that Alexander does not discuss the symbolic aspects of autocracy to clarify further the role of these men in Catherine's reign. I should hasten to add, however, that he does bring up the possible relationship of the choice of lovers to the interplay of court factions and patronage systems, whose significance for eighteenth-century Russian government has been well demonstrated in David Ransel's pioneering study and confirmed by the work of Brenda Meehan-Waters and John LeDonne.⁶

Roderick McGrew's primary purpose in *Paul I of Russia* is to paint a full portrait of Paul and to give an account of the coup that put an end to both his reign and his life. In that, McGrew is eminently successful, even though it is to be deplored that he gives but superficial treatment to the emperor's policies. McGrew quite rightly dismisses the question of Paul's alleged madness (in modern clinical terms) as being incapable of conclusive proof or disproof.⁷ But he does underscore and document the impulsive, unpredictable, suspicious, neurotic (in modern parlance) features of Paul's character; traits that rapidly intensified after he ascended the throne and justified Frederick II's forebodings upon meeting the grand duke, presumptive heir of Catherine II. McGrew also stresses, correctly in my opinion, Paul's "militarism"—his predilection for rigid order and central authority—as well as the didactic thrust of many of his actions. In this respect, he was genuinely heir to Peter the Great and the opposite of his mother, Catherine II, who relied (because she was a woman?) on creating consensus and cajoling elite society into cooperation. Furthermore, McGrew believes—and gives persuasive illustrations—that Paul's resort to the practices of the well-ordered police state and his strictness toward members of the court and officers of the guard regiments made for the unpopularity and fear that were at the root of the conspiracy's easy success and his successor's early popularity.

All three authors agree that the conspiracies that put an end to the reigns of Peter III and Paul I cannot be satisfactorily explained by the policies the monarchs pursued. Nor can it be shown that the groups in society allegedly supporting the coups had a direct interest in a change of reign, for there clearly was continuity in basic government policy throughout the century. (We shall return to this important issue.) It also seems that competition among cliques and patronage networks is inadequate to account for the coups against Peter III and

⁶ David L. Ransel, *The Politics of Catherine's Russia: The Panin Party* (New Haven, Conn., 1975); Brenda Meehan-Waters, *Autocracy and Aristocracy: The Russian Service Elite of 1730* (New Brunswick, N.J., 1982); John P. LeDonne, *Ruling Russia: Politics and Administration in the Age of Absolutism, 1762–1796* (Princeton, N.J., 1984); J. P. LeDonne, "Ruling Families in the Russian Political Order 1689–1825," *Cahiers du monde russe et soviétique*, 28 (1987): 233–322.

⁷ Compare Hugh Ragsdale, *Tsar Paul and the Question of Madness: An Essay in History and Psychology* (New York, 1988.)

Paul I, for the networks system was an extremely unstable one within a stable framework of a small governing elite; constantly changing court factions and family alliances were a permanent feature and did not, by themselves, bring about conspiracies or coups, as is readily shown by the long reigns of Elizabeth and Catherine, in spite of the kaleidoscopic changes at court. Nor do the three biographies under review (as well as recent studies of the period) support the interpretation (popularized by V. O. Kliuchevskii's influential *Kurs russkoi istorii*) that assigns to the guard regiments the primary impetus, as well as the leading role, in bringing about violent changes of reign in the eighteenth century. True, support by the guard regiments was crucial for the success of any coup—as shown *a contrario* by the failures of conspiracies against Catherine—but support is far from leadership.

Both Leonard and McGrew conclude that the successful coups against their heroes are to be explained by the conspirators' ambitions (in the case of Catherine) or by their fears and personal hatreds (in the case of Paul's murderers). In 1762, it was the resolute ambition of Peter's wife to be sole and effective ruler of Russia that brought a hasty end to her hapless husband, although no doubt Catherine could secure the support of individuals and institutions.⁸ In 1801, argues McGrew, Grand Duke Alexander did not play a leading part in the conspiracy, even though he probably assented to his father's dethronement. P. von der Pahlen, military governor of St. Petersburg, who had personal grievances against Paul and who was afraid for his personal safety as well as for his position in the government, became the *spiritus rector* of the coup; he was joined by N. P. Panin, the Zubov brothers, and a few influential officers of the capital's garrison, all of whom had been pushed into the background by Paul's capricious suspiciousness.⁹ Since none of the coups brought about a significant, let alone revolutionary, change in policy, are we to conclude that the coups were unimportant in historical perspective? It would seem so at first glance. But, in an autocracy, a change of monarch, especially when it is an unexpected and violent change, cannot but have an impact on the image of the ruler and repercussions for his or her authority.

This aspect, heretofore not addressed by historians, also suggests that an autocrat's individual comportment—especially when it does not conform to the ruling elite's expectations—becomes a crucial factor in the stability and success of the reign.¹⁰ In the eighteenth century, a negative evaluation of the sovereign's comportment led to his violent overthrow; in the nineteenth century, however,

⁸ *Pro domo mea*, I might note that while Carol Leonard rejects my argument that the displacement by Peter III of the senate as primary policy-making body contributed to his overthrow, she herself notes that under his reign, the senate was deprived of the power and influence it had under Elizabeth and that it recaptured them, at least temporarily, under Catherine II.

⁹ In a forthcoming book, *Zaveshchanie Ekateriny II*, M. M. Safonov mentions that in 1797 the Zubov brothers seem to have organized a conspiratorial circle in Smolensk (near their estates, to which they had been relegated by Paul I)—a circle that was perhaps also connected with the Masons in Moscow and St. Petersburg. In a conversation with me, Safonov also expressed his strong belief that it was P. Zubov who was the real guiding spirit in the conspiracy against Paul and that von der Pahlen only acted as his instrument and screen.

¹⁰ I understand that this theme will be treated extensively in Richard Wortman's forthcoming book.

untraditional and non-conformist comportment was avoided by means of strict patterns of upbringing and ceremonial—and there were no coups. It might be argued, of course, that at a time when a routinized pattern had become the norm, Nicholas II's inability to perform as expected, because of the irregular influence wielded by his wife, courtiers, and healers, played a major role in his loss of support and downfall in 1917. Be that as it may, in the case of the two violent coups of 1762 and 1801, it is a notable fact that the population at large—as far as we can infer from very inadequate documentation—not only remained passive but expressed sympathy for and attachment to the fallen rulers. This is indirectly illustrated by the many pretenders who called themselves Peter III and by soldiers and peasants expressing the opinion that Paul was a just tsar for them. What the significant symbols of the Russian monarchy were and how a sovereign was expected to behave are questions that deserve investigation on the model of studies of royal symbolism in the Middle Ages and in modern France and England.

EVEN THOUGH THEY FOCUS ON THE BIOGRAPHICAL ASPECTS of the three autocrats in the second half of the eighteenth century, these three authors strongly suggest continuities of policy in spite of the conspiratorial violence involved in each change of reign. When we consider the policies—in particular, the domestic ones—we usually speak in terms of intended or implemented “reforms.” I wonder, though, whether by using the term “reform” we do not imply purposeful legislative activity that aims at thoroughgoing transformations in the system. But this was hardly the objective of the autocrats’ policies. Along with the pursuance of so-called state interests in matters of foreign and fiscal policy, the governments of all three monarchs endeavored to raise the efficiency of existing institutions and the productivity of the country’s economy, as well as to ensure domestic tranquility. Thus it may be more appropriate to speak of adjustments and improvements in the workings of the administrative machine. Recent research on the period, especially on the long reign of Catherine II, has provided abundant material for teasing out and interpreting the main intentions and dynamics of imperial policy. The reign of Peter the Great set the direction for governmental action throughout the eighteenth century. Whenever the subsequent rulers (and their governments) were perceived as working toward the fulfillment of Peter I’s long-range goals, they received approval from contemporary publicists (and later historians). In a recent article, Cynthia H. Whitaker has cogently argued that in these cases the sovereign was considered to have adequately done his (or her) job and was termed a “reforming tsar,” whereas in the contrary case, he was dubbed a tyrant or incompetent; and, in the eighteenth century, such a reign was short lived. Vocabulary apart, Whitaker’s interpretation has much to commend it and shows that, in the eighteenth and early nineteenth centuries, Russian articulate society implicitly expected a ruler to be “activist” and “reform minded” (in line with the ideals of a well-ordered police state?).¹¹

¹¹ Cynthia H. Whitaker, “The Reforming Tsar: The Redefinition of Autocratic Duty in Eighteenth-Century Russia,” *Slavic Review*, 51 (Spring 1992): 78–98.

Carol Leonard gives a detailed and much-needed account of the legislative measures taken in the short reign of her hero Peter III, measures that proved important in the long run as well. In so doing, she greatly contributes to our knowledge and better understanding of Russia's evolution in the eighteenth century. Her description and analysis make it clear that the laws and decrees promulgated over the monarch's signature had been under discussion for a long time and were, in fact, prepared during the reign of Elizabeth; although, in some particulars, the laws' thrust was slightly changed by Peter's administration. Moreover, the full effect of the measures taken in the first half of 1762 was felt only during the reign of Catherine II, who, despite her aversion to her husband and predecessor, continued his economic and social policies. The telling difference between Peter III and his wife and successor—a difference that also distinguished her rule from those of Elizabeth and Peter the Great—was one of method. Where Peter III, like his grandfather, the first emperor, merely commanded, Catherine took care to elicit the opinion and muster the support of those groups in society she believed would count most in implementing her decisions.

Peter III, guided and assisted by Volkov, introduced a series of economic measures that, according to Leonard, were inspired by physiocratic notions and cameralist practices. They aimed at stimulating manufacturing and commerce on the part of the merchant class and other urban groups and at opening local trade to active participation by the peasantry. The policy was a marked departure from that of Elizabeth, who, under the influence of P. Shuvalov, had tried to involve the nobility, in particular the wealthiest court-connected dignitaries, in monopolizing economic entrepreneurship. To my mind, as suggested earlier, Leonard makes too much of Peter III's alleged abandonment of traditional mercantilism in favor of physiocracy, a policy that she believes Peter decided on and carried through personally. As far as the impact of the emperor's economic policies and theories is concerned, her evidence is at best circumstantial. But there is no question that Peter—assisted or guided by Volkov—did stimulate productivity and involve the non-noble classes in an increasingly dynamic economic process. Catherine II was not going to change this orientation; quite the contrary, she widened its application. An economic policy aimed at expanding the productive potential of the empire's population was well in the tradition of Peter I, while its physiocratic (that is, agrarian) bent was as much an all-European trend as Peter III's conscious implementation of what he (and Volkov) may have read in contemporary books and treatises. This brings up the question of the intellectual climate and cultural interests prevailing among the emperor's entourage. Leonard mentions some titles that Grand Duke Peter Fedorovich may have read, as well as his interest in music and military architecture. But the first achievements of a modern—or Europeanized—Russian culture occurred in the reign of Elizabeth, and it would be useful to have a fuller description and analysis of the cultural scene in mid-eighteenth-century Russia.

Upon his accession, Peter III took the dramatic step of pulling Russia out of the Seven Years' War and of initiating negotiations for a treaty of alliance with the erstwhile enemy Frederick II of Prussia. This step was later denounced by

Catherine as a betrayal of Russian national interests and a callous disregard of the sacrifices made, and of the victories won, by Russian troops. Leonard convincingly demonstrates that Russia's financial exhaustion had led more than a few dignitaries in Elizabeth's time to suggest withdrawal from the war. In any event, peace was widely welcomed in the army and experienced as a relief by the country at large. As to the alliance with Frederick II, it meant a return to the previous anti-Austrian (and anti-French) and pro-Prussian policy that Russia had consistently pursued since Peter the Great. Such a foreign policy was deemed to be in the empire's best interests (and determined by geopolitical factors, as LeDonne has argued). Not surprisingly, Catherine II maintained a pro-Prussian orientation not only during the preeminence of Count N. I. Panin and his Northern Policy but even after taking Potemkin's advice for aggressive expansionism in the south and agreeing to the partitions of Poland.

Peter III's government took two dramatic steps with respect to social policy: secularizing the monastic and episcopal lands, which included transferring the peasants working them to the category of state peasants, and freeing the nobility from compulsory state service. Both measures had been under discussion in the reign of Elizabeth, and both policies were maintained by Catherine II (although in the case of secularization, Catherine first abrogated Peter III's decree, only to implement her own more comprehensive measure in 1764). Far from being the act of a monarch who allegedly had remained Lutheran at heart and who despised Russia's religion, ritual practices, and clergy, Peter III's decree was the result of a long-term trend, and its terms had been the subject of intense discussion and preparation by the pious and bigoted Elizabeth.

In similar fashion, a change in the relationship between the noble service class and the state that would transform nobles into economically and culturally productive individuals with administrative responsibilities on the local level had been advocated and discussed by several committees during the last decade of Elizabeth's reign. Leonard gives a comprehensive account of these discussions—a subject that has generated much research and controversy. She concludes that the final decision and the terse act of February 18, 1762, ending the nobility's compulsory state service were taken personally by Peter, with the advice of Volkov. Even an exhaustive analysis of all available evidence does not permit us to reconstruct, with any degree of fullness and certainty, the role or motivations of various family and court networks, or even to pinpoint the influential personages in the final stage of the decree's drafting and promulgation. Leonard's account and analysis of the act exaggerates both the personal involvement of the sovereign and the underlying intellectual considerations (allegedly reflecting a clear understanding of contemporary social and economic theories). But she is perfectly right in viewing the "act of freedom of the nobility," as it came to be seen by both nobles and historians in retrospect, as a most significant step toward the goal of loosening state control over the elite of Russian society.

Leonard is not on quite as firm ground in her assessment of the decree's immediate impact on the residence and occupational patterns of nobles freed from the state service obligation. She argues that the act provided a solid base for the nobility's return to the countryside and for its members' greater involvement

in local and private affairs. Such a trend did develop, and it grew strong in the last decades of the eighteenth and the first quarter of the nineteenth century; but whether the process was entirely due to the act of 1762 is difficult to assess. The evidence adduced by Leonard belongs to the second half of Catherine's reign; by this time, however, other trends had also taken hold. Be that as it may, the act liberating the nobility from compulsory state service was a major step in realizing Peter the Great's ultimate goal of creating an active and productive Europeanized elite, the mainstay of a powerful empire in the European state system.

Whereas Leonard's positive account of Peter III's policies is much more satisfactory than her efforts at rehabilitating the emperor as a person, the opposite is true of John Alexander's treatment of Catherine II. He stresses her personality and private life, but his treatment of her accomplishments as a ruler leaves much to be desired. True enough, the book contains a reasonably full and accurate chronicle of all events and acts—both domestic and foreign—that have made Catherine's long rule such a seminal one in modern Russian history. Yet Alexander merely lists causes and consequences; he is not interested in distinguishing long-range trends and contingent or accidental factors, nor does he confront the perennial problem of goals and achievements. This is a curious approach for a historian who has written two solid monographs on the period, on the Pugachev rebellion and the plague crisis, in which he does precisely that. One can be a positivist and believe in pure pragmatism to account for actions; but, even then, one needs a conceptual framework to distinguish pragmatism and *ad hoc* decisions from actions determined by some overall system of values (political, economic, philosophic) and goals. Otherwise, we are presented with a bland listing of policy acts, each of seemingly equal importance. It creates an impression of mindlessness, which is not at all the case with Catherine's legislation. Her pragmatism was informed by clear ideas about goals and values, as has been well shown by de Madariaga.

Alexander underplays two equally vital aspects of Catherinian domestic policy: first, the long-range purpose of her legislation was to bring about a society of estates whose members would be secure in their status and property and free to pursue private concerns, while the elite would become willing helpmates of the state in the administration of local, economic, and cultural institutions. It is this long-range goal that accounts for Catherine's issuance of her three charters to the nobility and the towns (1785) as well as the projected, but not implemented, charter for state peasants (partly put into effect in the newly acquired province of New Russia). A pity that David Griffiths' masterful analysis of the thrust of the three documents evidently came out too late for incorporation into Alexander's book (although a preliminary draft appeared in 1989),¹² but some of the basic ideas had been presented long before in works obviously known to Alexander.

Second, an equally important feature of the empress's reign was what may be called the "immanent dynamics" of the empire's institutional framework. The administrative institutions that had been put in place by Peter the Great, and

¹² David Griffiths, "Introduction: Of Estates, Charters and Constitutions," in *Catherine II's Charters of 1785 to the Nobility and the Towns*, David Griffiths and George E. Munro, trans. and eds., *The Laws of Russia: Series II, Imperial Russia* (Bakersfield, Calif., 1991), xvii–lxix.

expanded and improved (or modified) by Catherine II, had generated their own concerns and interests. Their complexity and interdependence led the empress to introduce new local administrative offices and to define their relationship to the central bureaus of government in the Statute on the Provinces (1775) and subsequent additional decrees. The fiscal, military-logistic, and geopolitical aspects of this legislation have been well studied by LeDonne, whose work, I believe, Alexander tends to dismiss too cavalierly.¹³ The question of social organization along estate lines was, of course, implicit in Peter III's decree on freedom from service, and it was to be taken up again, in a different spirit, by Paul I. The efforts, stretching over practically the entire history of imperial Russia, at (re)structuring Russian society along European (modern) lines,¹⁴ raise the historiographic problem of continuity and discontinuity that is addressed by both Leonard and McGrew but not given sufficient attention by Alexander.

The reign of Catherine II witnessed the most significant expansion of the empire in the eighteenth century. The extension of Russian sovereignty over large areas inhabited by non-Russian peoples entailed not only diplomatic and military moves (the most important of which Alexander chronicles briefly) but also raised a number of administrative and socioeconomic issues concerning the non-Russian population, that is, what we would call the nationalities question. Alexander does not tackle this issue at all, but neither do Leonard and McGrew. The question has been of growing interest to historians in recent years, and valuable first steps to sharpen analytical concepts and elucidate specific facets of it in the eighteenth century have been taken by such scholars as Zenon Kohut, David Saunders, Edward Thaden, and Andreas Kappeler, to cite but a few whose work is easily accessible.¹⁵

I have mentioned in passing, in reference to Leonard's book, that the Russian elites and government administrators (from the sovereign down) acted within a climate of opinion shaped by their experiences since Peter the Great and by the increasing impact of West European ideas and values. Unfortunately, the three authors under review are oblivious to this facet of eighteenth-century Russia, except for the bare mention of cameralism, physiocracy, and the Enlightenment as formative concepts in the thinking of the three monarchs of their concern. These concepts, however, are neither spelled out nor their role in shaping the culture and ideologies of Russia's educated elites assessed. It was particularly desirable to do so in the case of Catherine II and her reign, for policies were motivated or even shaped by intellectual and cultural considerations. Yet in

¹³ LeDonne, *Ruling Russia*, and his more recent book, John P. LeDonne, *Absolutism and the Ruling Class: The Formation of the Russian Political Order, 1700–1825* (Oxford, 1991).

¹⁴ Gregory L. Freeze, "The *Soslovie* (Estate) Paradigm and Russian Social History," *AHR*, 91 (February 1986): 11–36. In a recent article in *Annales*, Michael Confino shifts the problem by arguing that Russia was "une société d'ordres" in the French *ancien régime* meaning of the term. Michael Confino, "Servage russe, esclavage américain (notes critiques)," *Annales: E.S.C.*, 45 (September–October 1990): 1119–39.

¹⁵ Zenon E. Kohut, *Russian Centralism and Ukrainian Autonomy: Imperial Absorption of the Hetmanate 1760s–1830s* (Cambridge, Mass., 1988); David Saunders, *The Ukrainian Impact on Russian Culture, 1750–1850* (Edmonton, 1985); Edward C. Thaden, *Russia's Western Borderlands, 1710–1870* (Princeton, N.J., 1984); Andreas Kappeler, *Russland als Vielvölkerreich: Entstehung, Geschichte, Zerfall* (Munich, 1992).

Alexander's book, except for his mention of Masonic lodges, we learn nothing about the modern Russian culture's coming of age that occurred at this time.

McGrew, focusing on the biographical side, deals rather superficially with the policies of Paul's four-year-long reign. McGrew makes clear (as LeDonne has argued more forcefully in his latest book, which may have come too late to McGrew's attention) that at the end of Catherine's reign, administrative inadequacies had become obvious and unmanageable, especially her attempts at decentralizing administrative authority and devolving more tasks and responsibilities to local institutions. It was, therefore, not simply out of contrariness and hatred of his mother's ways that Paul reversed some of her policies. True, he was by temperament in favor of central control and hierarchical organization on the military model. With respect to form and manner, in his actions he was clearly the descendant of Peter the Great and not of his mother. He shared with the first emperor a preference for didacticism and state direction—in short, he preferred a well-ordered police state to a civil society. Such an anachronistic throwback to the first decades of Russia's "Europeanization" and "modernization" had to arouse discontent and opposition. Combined with Paul's impetuosity, his capricious changes of mood and interests at the expense of sober consideration, the emperor's measures produced a sense of insecurity among leading circles in St. Petersburg and helped lay the groundwork for his overthrow. Whatever Paul's good intentions and useful measures (for example, encouraging regularization of serfs' obligations, creation of a noblemen's bank that profited serf owners, calling landowners and officials to account, promoting economic development and fiscal responsibility), they floundered on the reefs of Paul's erratic behavior and the perception that he intended to stop or even turn back the century-long trend that was transforming Russia into an up-to-date European polity.

THESE THREE AUTHORS PRESENT TELLING EVIDENCE that the heritage and goals of Peter the Great were the central factors of Russia's history in the eighteenth century. His aim of "transfiguring" Russia and its society provided the underlying dynamic force throughout the period; to the extent that a ruler adjusted policies to implement Peter's legacy, he or she would be "successful" and remain on the throne as long as the laws of physiology allowed. In this narrow sense, Cynthia Whitaker is quite right when she argues that only a "reforming tsar" validated the legitimacy of his or her power.

By the same token, a historian has to take into consideration the means available to the rulers in the performance of their tasks of government. And historians need to know a bit more than we do at present about the material, human, and cultural (or intellectual) resources at the autocrats' disposal as the complexity of society, economy, foreign relations, and cultural sophistication became ever greater in the course of the eighteenth century. We cannot do justice to these complexities without taking into account the factors of integration of stability—in contrast to the traditional emphasis on conflict and struggle (destabilization). In

this respect, the works of Michael Confino, Arcadius Kahan, and LeDonne have been very valuable,¹⁶ whatever one's reservations about their specific interpretations or concepts.

In the face of the disruptive dynamics of Petrine Europeanization, church and religion have provided an important force for cultural stability and social integration. It is an area of Russian life that is neglected by our authors, however (except for the discussions of secularization). The intimately related domain of so-called popular culture (and there may be a question as to how much most of the elite did in fact participate in it), in line with the erroneous beliefs and hopes of nineteenth-century radicals and twentieth-century Soviet historians, has been seen from the perspective of dissent and revolutionary potential. Quite correctly, of course, the process of cultural westernization has been seen as affecting primarily the educated elites. But has the rift with popular culture not been exaggerated? In the long run, cultural "Europeanization" did provide the element of *intellectual* continuity that lay behind the elites' aspirations toward administrative reform (in the primary sense of the word) and social improvements. Recent studies in the former Soviet Union have pointed to such continuities and acknowledged non-radical reformism as a force in shaping the attitudes of public opinion and civil society.¹⁷ There may have been much more agreement than we think between reform-conscious spokesmen of the elites and educated representatives of rural and urban commoners. Did not this facet of Russia's (political) culture in the eighteenth century create the intellectual climate that helped shape the goals and actions of administrators, courtiers, as well as publicists and clergy?

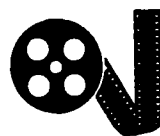
Research on this nexus of issues will surely lead historians to revise their conceptions of the nature and role of the intelligentsia. Did the so-called first *intelligenty* (Alexander Radishchev, Nikolai Novikov, and others) in the last decade of the eighteenth century truly represent a cultural movement? Most writers dealing critically with the manners and values of Russian society in this period, as well as the panegyrists of Russian national achievement, were neither dissidents nor potential revolutionaries. Should we not now study their words—written and spoken—from the perspective of their constructive integrative function as well? It may reveal a political discourse that has been ignored but one that could help us understand the intellectual scene of Alexander I's reign, and even the Decembrist movement, in a new way.

In conclusion, let me observe that a critical analysis of the biographical approach to Russian autocrats of the eighteenth century yields suggestions for further research as well as corrections and reinterpretations. The same would also be true if we had satisfactory biographies of the leading personalities in Russia's political establishment and cultural life in the second half of the eighteenth century. We do have biographies of literary and artistic figures, but we need studies of the rulers' helpmates such as the Panins, Volkovs, Vorontsovs,

¹⁶ Michael Confino, *Domaines et seigneurs en Russie vers la fin du XVIII^e siècle: Etude de structures agraires et de mentalités économiques* (Paris, 1963); Arcadius Kahan, *The Plow, the Hammer, and the Knout: An Economic History of Eighteenth-Century Russia* (Chicago, 1985); LeDonne, *Ruling Russia*.

¹⁷ The works of Safonov and Mironenko cited in note 3.

Bezborodkos, Viazemskiis, and many others. Biographically oriented studies may also turn up material to document the *acta* and *scripta* of less distinguished persons in all walks of life. But to have lasting value, such biographies need to be set in their proper cultural, social, and political contexts; only then will they shed new light and afford insights into the public life of Russia and lead to a much-needed (re)assessment of the dynamic trends and processes that shaped Russia's destiny in the eighteenth century.



ROBERT A. ROSENSTONE

Contributing Editor

AFRICA

Lumumba: Death of a Prophet. Produced and directed by Raoul Peck. 1992; color; 69 minutes. French, with English subtitles, and English. Distributor: California Newsreel, 149 Ninth Street, No. 420, San Francisco, Calif. 94103 (415) 621-6196.

Afrique, Je te plumerai [Africa, I Will Fleece You]. Produced and directed by Jean-Marie Teno. 1992; color; 88 minutes. French, with English subtitles. Distributor: California Newsreel.

Filmmakers in sub-Saharan Africa, especially francophone areas, realize that motion pictures reach a much larger audience than printed materials in that predominantly non-literate region. The inaccessibility and high cost of books also contrast sharply with the availability and immense popularity of movie theaters, present in virtually every African town. Given these films' highly charged historical and political content, however, as well as the current volatile situations in Zaire and Cameroon, neither work will likely be seen soon in those countries. African leaders understand the power and reach of visual representation and quickly ban controversial films. *Lumumba: Death of a Prophet* and *Afrique, Je te plumerai* strongly convey the need for political and economic change by directly linking postindependence African leaders with their colonial predecessors.

The parallels between the independence struggles in 1960 and current reform movements are striking and deliberate in the two films, making them timely and relevant.

Both films are visual essays, providing personal reflections on history, politics, journalism, and society. These are not heavy-handed, obvious propaganda features but sensitive and provocative deconstructions of colonial and postcolonial Africa. The directors clearly sympathize with the reform movements sweeping the continent. Many of Africa's current leaders, largely kept in place because of Cold War geopolitics, have continued the corruption, nepotism, and economic devastation inherited from colonial rule. These films offer an internal perspective on colonialism and its legacy, and both call for African initiative, unencumbered by outside intervention, to instigate and accomplish change. To varying degrees, both films portray Patrice Lumumba, a hero to many people but anathema to most governments, as a model for Africa's new leaders.

The more complex and original work, *Lumumba: Death of a Prophet* revolves around the unexpected and hasty independence in 1960 of the Belgium Congo (later, Zaire) and its first

prime minister, Lumumba. Haitian-born director Raoul Peck spent his youth during the 1960s in Kinshasa, where his parents worked for the newly independent government. He combines family home movies and photographs with archival documentary footage, intertwining private recollections and public history. He also includes recent interviews with journalists who reported from the Congo and news clips of Lumumba and Colonel Mobutu responding to reporters. The result is a combination of private autobiography and public biography, with an impressionistic though powerful portrait of a visionary, Lumumba, and a chilling cameo of his destroyer, Mobutu.

Colonialism was particularly harsh in the Belgian Congo, where King Leopold II ruled over a vast personal empire in the heart of Africa eighty times the size of Belgium. The region's many natural and human resources were exploited ruthlessly for the benefit of the mother country but always in the name of civilizing the Africans. Graphic photographs of the mutilation and death inflicted on the Congolese by their colonial masters are juxtaposed with contemporary images of Belgians on the snowy streets of Brussels and on public transportation. This is a subtle reference to one of the most notorious forced-work projects in colonial Africa, the construction of the Ocean-Congo railroad, which caused the deaths of thousands of Congolese.

When the winds of change engulfed Africa in the late 1950s, the Belgians, who had made no effort to prepare the Congolese for eventual self-rule, decided to pull out quickly in the hopes of retaining some economic influence in an independent Congo. The presence of the only national Congolese figure, Lumumba, worried the Belgians and the Americans, who interpreted his anti-colonial and anti-Western rhetoric as proof that he was a communist with Soviet ties.

After narrowly winning a majority in the only election in Congolese history, Prime Minister Lumumba immediately faced a conspiracy of internal and external forces. The film is very good at portraying the anti-Lumumba conspirators acting in their own self-interest. The key figure in the opposition to Lumumba, Colonel Mobutu (later, General Mobutu Sese Seko), headed the military, the only well-financed and organized force in the country. He rapidly became the West's man in Leopoldville (later, Kinshasa). A constitutional crisis ensued shortly after formal independence in June 1960, violence erupted, and Lumumba called in the United Nations, but it was too late. Mobutu took advantage of the upheaval and had Lumumba, his mentor, arrested for subversion. After only two hundred days in power, Lumumba was imprisoned, not by the colonial powers but by his own countrymen. Lumumba "escaped," only to be recaptured by Mobutu's forces in January 1961 and assassinated. Lumumba instantly became a symbol of the independent, populist Africa leader crushed by neocolonial forces. His reputation has grown steadily and will only be enhanced by this film, which effectively and accurately chronicles this turbulent period.

Mobutu consolidated his control over the country and has ruled the Congo/Zaire ever since as a staunch anticommunist despot. Despite the end of the Cold War, Mobutu still receives massive Western support. Reformers in Zaire protest against Mobutu and carry pictures of Lumumba as their hero. Anti-government leaders in other countries also emulate Lumumba. This film is an important reflection and reexamination of the fall of Lumumba and the rise of Mobutu during the independence struggle a generation ago.

Rather than creating the familiar, straightforward narrative of most film biographies, Peck manipulates time and chronology in the manner of an African storyteller or oral historian. Oral traditions from Africa, and their closely related modern counterparts, films, often deal with historical and political events by focusing on one figure, or hero, and relating the person's triumphs and defeats through a mixture of narrative, song, anecdote, and dialogue. The concern is not with telling a highly structured, unfolding story from beginning to end but with creating a memorable and unique portrait. In *Lumumba*, the images and narration shift from public to private and from past to present without warning, evoking a spoken tradition with its various components and digressions. However, the rhythm of sight and sound is never disruptive. The voice-over is dispassionate yet reflective, delivered in a monotone, and often divergent from the images on the screen. The film succeeds admirably as a visual and oral meditation on Lumumba and his times and on the promise of African independence thirty years later.

Afrique, Je te plumerai takes a somewhat different approach in its questioning of the meanings of colonialism, neocolonialism, and independence in the African context, but it arrives at similar conclusions. Cameroon is the sole African country to have been colonized simultaneously by three European nations; France, Britain, and Germany, and yet its colonial and postcolonial experiences are remarkably similar to those of other African nations. The title, a takeoff on a verse from the familiar French song, "Alouette," sets the tone for this sardonic and often humorous essay on the history and legacy of colonial rule. Unlike Peck, Jean-Marie Teno was able to film and interview in Africa. Besides politics, the director focuses on historical and contemporary European cultural domination in Cameroon, particularly in publishing and other media. *Afrique* emphasizes that Cameroonians, and other Africans, must reclaim their cultural as well as their political and economic institutions in order to achieve true independence from Europe and the West.

One of the more interesting segments concerns an African woman in Douala who is visiting the British, French, and German cultural centers and the national television station seeking works by Cameroonian and African artists. Not surprisingly, she finds little that is African and much that is Western in a supposedly independent African country. The excuse given: books and television programs are produced in the West, which has the financial resources. The film then shows a thriving Cameroonian printing press in Douala and follows this with a discussion of government censorship. The situation is handled humorously but effectively. The publishing and media industries present a decidedly Eurocentric view of Africa to Africans. Teno's film identifies and takes a step toward correcting that bias.

Both films are intelligent, provocative, and timely. The directors experiment successfully with variations on the documentary form. Both filmmakers, but especially Peck, are aware of the similarities between film and oral tradition, and they draw on techniques borrowed from spoken history and storytelling. Although the messages are highly political, the directors never lapse into propaganda, nor do they make the obvious connections for their audience. They let the images and voices of the past and present speak for themselves.

Andrew F. Clark

University of North Carolina, Wilmington

ASIA

Raise the Red Lantern. Directed by Zhang Yimou. 1991; color; 125 minutes. Chinese with English subtitles. Distributor (film and video): Orion Classics, 304 Park Avenue South, 10th Floor, New York, N.Y. 10010 (212) 505-0051.

The Story of Qiu Ju. Directed by Zhang Yimou. 1992; color; 100 minutes. Chinese with English subtitles. Distributor: film, Sony Pictures Classics, 550 Madison Avenue, 8th Floor, New York, N.Y. 10022 (212) 833-8833; video, Columbia Home Video.

On a visit to China in the early 1970s, the American film actress Shirley MacLaine noted how impressed she was by the sight of Chinese women moving rocks and laying bricks in the Chinese countryside; the Communist Revolution had obviously transformed the seemingly

perennial oppressed status of women in China's feudal past. Ironically, Ernest Hemingway had long before commented on the physical prowess of Chinese women, mostly Hakkas, whom he saw engaged in quarry excavation on the Kowloon Peninsula in pre-1949 Hong Kong (albeit the work *Islands in the Stream* [1970] was published posthumously).

To be fair, MacLaine was not the only one affected by the Chinese propaganda machine at work. Even those who have devoted their lives and careers to the study of China were at one time swayed in various degrees by what they thought they saw and heard. Right before the Tiananmen massacre of June 4, 1989, the doyen of American sinology, the late John K. Fairbank, warned that American euphoria over China, including his own, generally had had something to do with the "charm" of the Chinese. And in his last published work, *China, A New History* (1992), Fairbank conceded that sentimental sinophilia might have led scholars to "want to say no evil of the object of their researches." Specifically, he was referring to the lack of studies on footbinding—"a fact with causes and repercussions still to be understood"—in China's imperial past.

Other than the usual anthropological and sociological treatments, the study of women in Chinese history has yet to be confronted by historians (Jonathan Spence's *The Death of Woman Wang*, 1978, is perhaps a rare exception). We may know plenty about arranged marriages or how the subjection of women was institutionalized, but, more often than not, Chinese women are depicted as one-dimensional beings: victims of a male-dominated social order who occasionally find escape by inflicting pain on others. The classic figures are the Empress Dowager Cixi and Madame Mao, Jiang Qing. But how did their inner worlds look? What were their psychological orientations, especially toward the Chinese male? Were they cunning, suspicious, and deceitful because these are traits of the Chinese female psyche?

These two films by Zhang Yimou (like his earlier films *Ju Dou* and *Red Sorghum*) provide some of the answers, partly because Zhang approaches his subject—Chinese women—with great sympathy and appreciation (in fact, the leading female roles are played by his real-life lover Gong Li) and partly because he is bold enough to strive to re-create the whole picture by being honest about issues both past and present. At the same time, all his films imply that the Chinese world is cyclical and that the past with its multifarious layers is inherently part of daily living today.

In his attempt at historical reconstruction, it is therefore not surprising that, in approaching the dynamism of Chinese women, Zhang explores dangerous and uncharted territory (at least from the perspective of contemporary Chinese orthodoxy as defined by the state) in order to help the audience visualize the effects of China's traditional practices in maiming half its population. One avenue Zhang particularly favors, and this is done with deliberate intention, is the portrayal of his main characters as consumed by sexual matters, a socially and politically taboo subject in Chinese films.

Raise the Red Lantern, based on the short story "Wives and Concubines" by Su Tong, a "new wave" Chinese mainland writer, starts out as an ordinary tale of an impoverished family with one means of survival: to allow its eligible female to become a rich man's concubine. But Songlian, the protagonist, is no typical Chinese girl. She has had one year of university education. She is also extremely strong-willed; it takes her mother three days to persuade her to marry for money. She finally relents because, as she puts it, "such is a woman's fate."

Still, she tries to maintain her individuality by engaging in rebellious behavior: on her wedding day, she avoids the bridal sedan-chair and travels on foot to her new master's household. She meets the other wives with dignity and with a sense of comradeship. As the number three concubine, she soon discovers that rules and traditional customs are degrading and debilitating, to say the least. But these "family laws" are powerful weapons. Chinese women were willing to put up with so much for so little probably because the reward for obeying was irresistible; it is the addicting practice of foot massage (not dissimilar from opium smoking) that eventually crumbles Songlian's will to resist. Perhaps foot massage and footbinding had similar erotic functions; at least, the master believes that a good round of the former will turn his women into adequate tools of pleasure. On the first night, he also insists on a brightly lit room to add extra excitement to his indulgence as he claims his newly acquired property.

Suffice it to say that foot massage is only one part of a larger package: all the wives have to stand to attention every evening at the entrance to their respective quarters, and only one will be graced—a red lantern is raised to signify the master's choice. The next day, the lucky one even has a say over the choice of dishes for the family meal. Here, Zhang offers an explanation for obedience that helps the audience truly understand the clichés of Chinese iconoclasm that began with the May Fourth (1919) anti-traditional stand “Down with Confucius and Sons.” Because both their minds *and* their bodies were controlled by whoever spoke in the name of tradition, Chinese women did not rebel. Even a concubine's murder was permissible should a particular female act as if she had control over her own body. Concubine number two, for instance, was hanged because she had committed “adultery” with the family physician.

On the surface, a challenge to the labyrinthine Chinese bureaucracy by a female peasant is the main theme of *The Story of Qiu Ju*. The heroine may not have been so persistent, however, if her husband had not been kicked in the groin in the first place. Admittedly, this is an allegorical tale on a Chinese female's quest for justice, yet Qiu Ju's obsession with her husband's reproductive organ helps to highlight the village chief's “moral” dilemma—his inability to have a male heir. His difficulty is further compounded by the Communist regime's prohibition of polygamy. New laws in an ancient land; everyone pays a price for bringing egalitarianism to the New China.

Qiu Ju wanted a public apology to satisfy her sense of justice, and from a man—an older man with authority. Her husband and a whole host of Chinese officials asked her to accept monetary compensation and let the incident drop. But Qiu Ju had a larger claim; her yin and yang world had been upset by an “insult” to her husband's “ancestral temple,” and she wanted a verbal acknowledgment of that fact. The only support she received came from her sister, who traveled with her on every trip, slept anywhere, and ate whatever food Qiu Ju gave her. She was ignorant (especially of city life, i.e. modernization), but she was completely loyal. A Chinese female virtue, perhaps?

By contrast, the “sisters” of Songlian found psychological satisfaction in undermining one another's status and well-being. Instead of directing her anger at her husband, each wife tried to take out her personal frustrations by increasing the misery of another. There was no sisterhood here, although interestingly enough, sometimes their squabbles did irk the “faceless” master.

But the relationship that gives a real sense of the tragedy of women in Chinese society is the one between Songlian and her personal maid, Yan'er. From the very start, this house slave was jealous of Songlian because she thought she had a chance at becoming the next concubine. In turn, Songlian knew that there were physical contacts between Yan'er and the master but decided that the other concubines were more formidable enemies—until the slave betrayed Songlian's false pregnancy. The eventual death of Yan'er, indirectly caused by Songlian, exposes the brutality of a sado-masochistic Chinese order—those caught in the web of its feudal traditions were destined for violent ends and madness.

In “old” China (which still exists today, Zhang constantly reminds his audience), there were few alternatives for women. They either killed themselves (suicide of women is another rarely explored subject in traditional China studies) or had others killed. The adventure of Qiu Ju, an upbeat one, however, shows that Zhang—after his psychological probing of the inner world of the Chinese female—still insists that the hope of a revitalized China must include an awakening of its womenfolk. Critics (especially his compatriots in Hong Kong and Taiwan) have charged that Zhang, in conveying the message that justice can still be found in China's judicial system, is being an apologist for China's present regime. Some have even suggested that his films are made for foreign consumption: Chinese women simply do not petition against senior Communist cadres, for whatever reason.

There may be hidden metaphors here and there, but basically these two films are not terribly concerned with the current state of politics in China. Songlian fought back and lost. Qiu Ju fought, won, and yet was not quite sure exactly what she had won. But both tried their utmost to maintain their humanity and sexuality (of a gentle kind) throughout: they never blamed others, least of all the men in their lives, for their own actions. Chinese women, these two films suggest, are at least brave enough to try and come to terms with the Chinese past.

There is no question that both films have made important contributions to our historical understanding of China. This is not to say that there are no historical flaws in or controversy over Zhang's representation of Chinese women. (It should be pointed out that both screenplays are based on already published material.) I have only concentrated on the more revealing aspects because the subject matter is so rarely dealt with in such a serious manner that Zhang should be congratulated for his pioneering efforts. (A few filmmakers in Taiwan in the mid-1960s tried to depict the role of women in Chinese society but rather unsuccessfully because of political constraints.)

Zhang, in his vivid accounts of Chinese women on film, has opened up all sorts of possibilities for the use of the visual media in China studies; along the way, he has demonstrated that in the portrayal of emotions and male-female interactions, written versions of the past need the supplements of sight and sound to tell a good and complete story.

John Dragon Young

University of Hong Kong

Rhapsody in August. Produced by Hisao Kurosawa; directed by Akira Kurosawa. 1991; color; 98 minutes. Distributor (film): Shochiky Company, Ltd.; Orion Classics.

This film, directed by one of the world's greatest living filmmakers, is no rhapsodic masterpiece. Rather, this affecting work is a quixotic summary of several of Akira Kurosawa's dramatic concerns during forty years of filmmaking. Like many of Kurosawa's recent films, *Rhapsody* suggests the impossibility of solving social problems through heroic action (see Stephen Prince, *The Warrior's Camera* [Princeton, N.J., 1991], 250–91). It thus lacks the optimism of Kurosawa's samurai epics or even *Ikiru* (1952), films in which the dedicated moral actions of single characters transformed the lives of those around them. *Rhapsody* is also a return to the theme of nuclear annihilation and terror, first explored by Kurosawa in *I Live in Fear* (1955). *Rhapsody* is more affirmative than that film or any of Kurosawa's other recent works. Yet it proves unsettling in its ambivalent treatment of the power of memory and in its evasion of human responsibility for horrendous events in the past.

Rhapsody focuses on Kane (Sachiko Murase), a grandmother living in the suburbs of Nagasaki who hosts four grandchildren on a summer visit. As the film opens, the family considers a significant dilemma: Kane's brother, Suzujirō, who long ago moved to Hawaii and whom she barely remembers, is dying and wishes to see her one last time. Suzujirō has grown wealthy as a pineapple magnate, and Kane's son and daughter, who have gone to visit, write from Hawaii urging Kane to come as well. Because a visit would cause Kane to miss an annual memorial service for her husband, she decides to recount to the grandchildren how the atomic bomb, dropped in August 1945, not only killed their grandfather but irrevocably changed Nagasaki and its inhabitants. Much of the film involves these recollections and the grandchildren's discovery of the history of Nagasaki.

Desperate to persuade his reluctant sister to come to his deathbed, Suzujirō next dispatches his son Clark (Richard Gere). Kane is startled to find that Clark is "an American" born to a Caucasian mother. His spoken Japanese is stilted and his written Japanese childlike. Yet Clark quickly charms Kane and establishes a rapport with the grandchildren. He cavorts with them in an improbably bucolic countryside and shares quiet hours of moon viewing with Kane. Moved by Clark's empathy, Kane just seems resolved to visit her brother when Clark learns from Hawaii that his father has died. Kurosawa undermines this poignant resolution with an ironic turn. Transfixed by a thunderstorm the day following Clark's departure, Kane relives the experience of the atomic bomb. She flees into the storm. Her children and grandchildren chase after her in panic, but she plods ahead, whipped by the rain, in demented determination.

Rhapsody explores the tension between the healing power of recollection and the curse of

memory. During their summer with their grandmother, the children are able to enter her world. She recounts, for example, the story of a water imp (*kappa*) who saved her younger brother from drowning and dragged him back home. The same brother, we are also told, later went mad from postnuclear trauma. Kane explicitly links the grandchildren with these magical past events: Shinjirō, the youngest child, reminds her of her mad brother. Initially shaken by the suggested resemblance, Shinjirō later exacts a humorous revenge, startling his siblings by dressing up as a *kappa*. Kurosawa builds on such associations between past and present and the traditional Japanese fairy tales throughout the film.

The most disturbing memory, of course, is Nagasaki. When the grandchildren learn that their grandfather died in the bombing, they speak bitterly of America and of their American extended family. On a day trip to Nagasaki, they visit the schoolyard where their grandfather, a schoolteacher, probably died. The wreckage of a postnuclear jungle gym, twisted from the heat of the explosion and resulting fire, seems to be a metaphor for lives racked with pain by memory. Grandmother does not wish to visit Hawaii, they conclude, because of an animus over her husband's death. They elect to avoid greeting Clark at the airport and instead escape to the schoolyard.

Yet the playground emerges as a site of reconciliation. Clark has come to Nagasaki eager to learn how his uncle died. Despite the consternation of his cousins (Kane's daughter and son), Clark insists on visiting the schoolyard en route from the airport. There he meets the grandchildren, and an eagerness to understand the tragedy of Nagasaki unites rather than divides Clark from his second cousins. Kurosawa's use of a schoolyard here recalls the symbolism of his immediate postwar films, in which the playground suggests the vitality of youth and, by extension, hope for the future.

Clark's reconciliation with Kane hinges on Nagasaki as well. As they sit watching the moon, Clark apologizes for his ignorance of his grandfather's death. This scene has invited misinterpretation. Among others, Vincent Canby of *The New York Times* has taken it as an apology for the bombing of Nagasaki ("Kurosawa, Small in Scale and Blunt," *New York Times*, December 20, 1991). The statement Clark makes is not unambiguous, and both the subtitles and Clark's broken, non-native Japanese accentuate the ambiguity. Yet, in the context of the film, the statement seems clear: Clark knew that his family was from Nagasaki. He had never considered, however, that his relatives might have been killed by the bomb. In pressuring his aunt to come to Hawaii, he had been unwittingly demanding that she miss her husband's memorial service. His apology is distinctly "Japanese," in that it is concerned less with guilt and moral abstraction than with shame and a concern for human sentiment.

Kurosawa, by contrast, is less forgiving of Clark's cousins (Kane's children), the immediate postwar generation. When they return from Hawaii, they speak only of the possibilities of joining their Hawaiian relatives' lucrative pineapple business. Their selfishness and greed recall that of the wealthy family in *I Live in Fear*. More broadly, Kane's children misread both Kane and Clark's motives and assume that Clark is coming to Japan to break off relations. "Americans don't like to be reminded of the Bomb," they reason, "especially Japanese Americans." Kane and Clark's reconciliation, which delights the grandchildren, leaves Kane's children puzzled.

Clark emerges as an embodiment of compassion and the renewal associated with the playground. Tateo, the eldest grandchild, labors endlessly to repair and tune Kane's old organ while his siblings tease him over the futility of his efforts. Nevertheless, when the grandchildren perform a song for Clark's arrival, the organ and the grandchildren are in perfect harmony. Kurosawa emphasizes the similarities between Clark and the grandchildren. Clark and Shinjirō ask the same kind of questions about Buddhist memorial practices. While playing guilelessly with his second cousins in the *kappa*'s pond by the waterfall, Clark learns of his father's death. Kurosawa abruptly cuts out the sound effects in this scene, as he did after the grandchildren's first visit to the schoolyard, thus associating Clark's suffering and loss with those of his Japanese relatives.

After such a poignant turn of events, Kurosawa pulls the rug out from under our feet. Hopeful as Clark's visit has been, the film now reiterates the pain of Kane's memories. As her children and grandchildren pack up to leave during a thunderstorm at the end of the summer,

Kane believes she sees her dead brother, whom she had struggled so hard to remember at the beginning of the film. She mistakes lightning for another bombing and runs out into the storm, with the children fruitlessly chasing after her. Kurosawa replays at high volume the song with which the grandchildren had greeted Clark, its naïve tone completely inappropriate to the action of the scene.

I Live in Fear and even *Seven Samurai* ended with epiphanies of futility. But the break here is so abrupt that its implications prove disturbing. Kane is privileged as the film's spokesperson and society's memory. At the end of the film, we find that memory, for all its benefits to the grandchildren, ultimately provides Grandmother not with an acceptance of the past but with a consuming form of torture. That hers may be the madness of the true insight is little comfort.

Other implications are equally perturbing. The memories preserved in *Rhapsody* are naïve and limited. World War II, for Kane, exists only in the form of the Bomb. When she remembers herself as one of eleven siblings, many of whom are now dead, none of her eight brothers seems to have left Japan to fight in China or Southeast Asia, a statistical improbability. The grandchildren notice that unlike Cuba, Bulgaria, and the People's Republic of China, America failed to send a memorial to Nagasaki's memorial park, because, they reason, it was the Americans who dropped the Bomb. The film is so focused on Japanese-American relations that it ignores the complex political motivations for the other countries to send memorials to victims of an American nuclear weapon. The Bomb exists outside of history.

Kurosawa's central concern is with the legacy of ambivalent feelings created by the war. But having divorced the Bomb from human history, Kurosawa interprets it as a natural and evil force, as unyielding to human reason as a water imp. America is released from "Bomb guilt" because the Bomb, like a waterfall or a rainstorm, is beyond the purview of human conduct. Kane explains that she does not blame the Americans for the bombing of Nagasaki. "War is to blame." Thus Clark and his Japanese relatives can play together happily. Kurosawa's study of the war's effects is distorted by his disturbing reluctance to confront its causes. Combined with the abrupt shift in tone during the film's epilogue, his conception of history finally undermines even the most moving themes of the film, those of compassion, reconciliation, and the social and natural harmony evoked by the film's title.

Matthew Bernstein and Mark Ravina

Emory University

How to Behave [*Cau Chuyen Tu Te*]. Produced by the Anh Hung Documentary and Scientific Film Enterprise, Hanoi, Vietnam. Directed by Tran van Thuy and Ho Tri Pho. 1987; color, 43 minutes. Vietnamese with English subtitles. Distributor: First Run/Icarus Films.

A dying filmmaker asks his colleagues to make a film on the theme of human kindness (*tu te*). To honor his wishes, his colleagues embark on a tour through postwar Vietnam.

If you are looking for an example of accomplished filmmaking, this is not the film to watch. Yet, for all its technical flaws, *How to Behave*, which was made shortly after the Sixth Party Congress of December 1986, is an important historical document. It was at the Sixth Congress that the Vietnamese Communist Party decided to make the transition from a centrally planned socialist economy to a market-driven one. This policy of renovation (*doi moi*) was accompanied by political liberalization, which in turn gave rise to a wave of dissent literature. Since 1989, government censorship has turned strict once again, and artists have been fired from their jobs or imprisoned for producing critical art.

How to Behave begins with an unattributed quote: "Only animals are capable of turning

their back on the suffering of human beings and busy themselves preening their furs and feathers." The statement is repeated at the end of the film, this time with an attribution to Karl Marx. But not even Karl Marx and Ho Chi Minh, also quoted in the film, were able to protect the filmmakers from the censors' wrath. That the film was made at all shows how much government control over the arts loosened after *doi moi*. That the film has since been banned shows how limited political liberalization turned out to be.

The film demonstrates that party officials and government bureaucrats have become remote from the people they are pledged to serve. A brickmaker shouts, "Why don't you show how ordinary people live for once instead of making up stories about us?" Children complain, "Your films put us to sleep." These observations lead to a discussion, illustrated with newsreels, of the complicity of artists in the production of state propaganda.

Another theme is the yawning gap between the rhetoric of glory and greatness and the reality of poverty. During the course of the film, various people are asked to define the meaning of *tu te*, a much-used word in Vietnam, which is translated as human kindness but can also mean decency, charity, and appropriateness. One example given is of a young woman who, abandoned by her husband and shunned by her community after contracting leprosy, built a brick house with her bare hands to provide a roof for her little boy in case of her death (she recovered). Another is of the Catholic nuns at the Lepers Hospital in central Vietnam. These are clearly meant as counterpoints to the illustrations of official indifference, even arrogance, toward ordinary people and the loss of commitment to socialist ideals.

The film offers a fascinating glimpse into (North) Vietnamese society at the start of economic and social change, such as veterans of Vietnam's thirty-year war against colonialism and imperialism now eking out a meager living driving pedicabs or repairing bicycles. The mouthpiece for the emerging consumer society is a sleek, self-assured young woman: "In practical terms, kindness nowadays means someone doing you a favor using either political influence or material wealth. Kindness is an old-fashioned idea that only older people like to talk about. Today, people are far too busy to discuss outmoded concepts like that."

If socialism is a failed social system and a spent spiritual force, what of the brave new world of free enterprise that is being created? Can it be that Vietnam's future under *doi moi* is to be seen not just in images of prosperity but also in the person of the prosperous young woman who sees kindness as just another commodity to be traded? Should it be a cause for rejoicing that socialism's failure to create the New Man devoting himself selflessly to the common good is leading to a generation of egoists all clamoring, "Me, me, me"? These dilemmas of the transition from socialism to capitalism, presented only in embryonic form in *How to Behave*, have become acute and urgent issues in Vietnam today.

Hue-Tam Ho Tai

Harvard University

EUROPE

Edward II. Produced by Steve Clark-Hall and Antony Root; directed by Derek Jarman (with assistance from Ken Butler). Screenplay by Derek Jarman, Stephen McBride, and Ken Butler. 1992; color; 92 minutes. Distributor: The Sales Company.

Derek Jarman's deliberate queering of Christopher Marlowe's *Edward II* (the published script in fact bears the name *Queer Edward II* [1992], although the adjective is absent from the title of the film itself) creates its gay-positive message by elevating the subcurrent of sodomy

in Marlowe's play from subversive subtext to textual center. It is in-your-face gay assertiveness that Jarman features: when, early in the film, Piers Gaveston reads Edward II's letter calling him back from exile, two nude men make love on the bed behind him. In this, the film can be taken as an example of recent scholarship in literary and cultural studies that treats Marlowe's homosexuality with new sympathy; for instance, Jonathan Goldberg's argument in *Sodomities: Renaissance Texts, Modern Sexualities* (1992), that sodomy functions as a site of resistance in Marlowe, is perfectly in keeping with Jarman's interpretation. For Jarman, Edward II's creation of a court life rooted in homoerotic aesthetics placed the king at odds with an aristocratic and church culture committed to the ideal of compulsory heterosexuality.

In addition, Jarman employs deliberate anachronism to create a postmodern pastiche that underlines the dichronic dimensions of his film, emphasizing the interplay between interpreting the past and understanding the present. The technique has long been standard in Jarman's repertoire, and it has always signaled a reappraisal of past aesthetics through an emphasis on homosexuality, as is most famously the case in Jarman's *Caravaggio* (1986). Since Jarman's own discovery that he is HIV-positive, the deployment of pastiche has gained a sharper and more acerbic edge. *War Requiem* (1988), Jarman's film version of Benjamin Britten's oratorio of Wilfred Owens's World War I poems, for example, underlines the homoerotic elements of military camaraderie and of Owens's aestheticization of trench warfare, while inserting an AIDS-aware subtext of exchanged blood. *Edward II* takes this project significantly further, imagining homosexuality as both the grounds for aesthetics (the masques promised by Gaveston as he returns from exile, left out of most stagings of the play but aggressively reintroduced in Jarman's version) and a major site of conflict in the court of the English king.

Edward II is not just a historical reevaluation of a text and period that insists that the questions we ask of the past are grounded in the present, it also uses history as a mirror of present conflict. The embattled gay court of Edward II offers an echo to current conflicts over gay rights and AIDS funding, and it echoes as well Jarman's own struggle with AIDS (severe enough at this point, he notes in marginalia to the published script, that some scenes were directed by his "ghost," Ken Butler). Jarman demands that the connections between past and present be made when he dedicates the film to the repeal of Section 28 (the recent English anti-gay law) and when he features in the king's final battle placard-carrying members of OutRage (the English version of ACT-UP) facing riot-equipped police.

Jarman works less against Marlowe's text (with minor updating and elisions, the film is faithful to the original) than against a tradition of interpretation and staging that reads Edward II's homosexuality as ineffectual effeminacy, the tragic flaw and weakness that necessitates his overthrow. Countering this interpretive tradition, Jarman envisions a homoerotic court society that has both its own aesthetic forms (ranging from dancers and string quartets to a naked rugby scrum) and its own community egalitarianism. Thus Edward II is never Gaveston's superior, for instance, but always his equal; the power hierarchies of heterosexual practice are undercut by the fundamental equalities of homoerotic desire. There is, further, nothing weak or indecisive in Jarman's Edward but rather a strength defeated only because it is faced by impossible odds.

Against this court are ranged the forces of hierarchical power: the lords, the church, the military, and the queen. Embodiments of small-minded homophobia, the lords and churchmen constitute an angry chorus of dourly dressed bigots, hateful in their insistence that old power relationships must not give way to the privileging of male friendship (as with the entitling of Gaveston as earl of Cornwall). Heterosexuality is never, in Jarman's vision, a partnership of equals or even a relationship of love. (This is even more the case in the printed script, in which slogans labeling each scene aggressively assert the virtues of homosexuality and the deviance of heterosexuality.) It is always, instead, about the exercise of power, embodied in Roger Mortimer's taste for sadomasochistic sex (he is pummeled, trampled, and leashed by leather-clad women even as he denounces the perversity of the court) and Queen Isabella's icy vampirism (she kills the earl of Kent with a bite to his neck). The coupling of Mortimer and Edward is emphatically political rather than romantic, a Machiavellian calculation on both their parts. When, Edward supplanted, Mortimer and Isabella pose for an official picture with the

young prince, the stiff poses and glaring light suggest the nuclear family as militant dictatorship. If the portrait of Isabella borders on the misogynistic at times, the main impact of Jarman's reconfiguring is to invert standard stereotypes, to insist on the unnaturalness of homophobia and of the other power hierarchies Jarman links to the heterosexual regime.

The narrative of Jarman's film is structured as fitful memory in the mind of the imprisoned Edward, a series of brief flashbacks as he awaits what he presumes to be his doom, framed by his soliloquies to himself and his silent jailer. The film opens in the rusting ship's hull that serves as Edward's dungeon, a starkly barren set that establishes the tone for the rest of the movie: minimalist sets establishing a tension with more elaborate accessories of the *mise en scène*, that tension further accentuated by the anachronistic touches. Jarman mixes past and present throughout, but he moves generally from a more period-piece approach in the early part of the film to an increasing employment of anachronistic details in the later scenes, especially in the final battle. The shifting chronology suggests that the real struggle is more contemporary than historical.

Jarman cannot let the triumph of heterosexuality through the overthrow of Edward II stand. He undercuts Mortimer's victory in two ways. First, he makes it clear that the young prince, who will in turn overthrow his father's foes, is truly his father's son: the camera catches the child trying on his mother's jewelry. In an almost hallucinatory scene near the film's end, we see Edward, now child-king, made up and bejeweled, dancing atop the cage that holds a desiccated Isabella and Mortimer. Second, he liberates Edward himself at the end. We see his jailer heating the poker for the famous execution by impalement; then, in a flash forward, Edward imagines his death; but, in a sudden turn, the jailer throws the poker away, and he and Edward kiss. The power of homoerotic desire thus frees the king once again.

In his notes to the printed screenplay, Jarman anchors that final escape on historical rumors: a letter from Martin Fieschi to Edward III, assuring the king that his father had escaped to Italy (*Queer Edward II*, 158) and, somewhat more dubiously, a diviner's assurance that Edward's tomb is empty (118). Given the film's dichronic nature, its address to two different times, the historicity of Edward's escape perhaps matters less than its contemporary meaning: the hope that one day a gay sexuality and culture, suppressed in Edward's time and still under attack in ours, can freely thrive; and, on a more personal level, the wish that the disease killing Jarman himself may one day kill no more.

Thomas Prasch

Indiana University, Bloomington

Lichnoe Delo Anny Akhmatovy [The Akhmatova File]. Directed by Semen Aranovich; written by Semen Aranovich and Elena Ignatova. 1989; color; 65 minutes. Distributor: Facets Video, Chicago (312) 281-9075.

The Akhmatova Story. Written, directed, and produced by Jill Janows. 1991; color; 60 minutes. Distributor (video): New York Center for Visual History, 625 Broadway, 12th Floor, New York, N.Y. 10012 (212) 777-6900.

Film biography displaces the delicate balance between subject and historical background without diminishing the significance of either. Interdisciplinary in domain and artistic in form, film presents a lustrous, essentialist version of history in which the subject, almost inevitably, is foremost. The fullness of its meaning, not of its detail, is the measure of the film as history. Semen Aranovich's study of an extraordinary twentieth-century Russian poet, born Anna Gorenko, later Anna Akhmatova, is an example of how well film can work.

Aranovich conveys Akhmatova's life by recovering her perspective and using it as the structure of the film. His visual images reflect the lightness and delicacy of her poetic images. The music, sometimes airy, is dissonant and disturbing. The narration moves forward by encounters of all kinds that mirror the encounter structure of her poems. Aranovich's text is powerful and sparse, consisting entirely of her writing (poems, letters, and notes) and of reminiscences by Lydia Chukovskaia. Space limits the shaping conditions the director can convey, and so he highlights only a few of the most important, as she did in her notes. Aranovich gives Akhmatova a real presence, in documentary clips of her declaiming her poetry, and a voice, in superb readings in Russian by Rosa Balashova and Larisa Malennaia.

This poet's life in film perspective overlays the late and the early Akhmatova. The film assigns her the part of an older woman, reminiscing about the past as if in an interview. It is an effective device, somehow persuasive of historical distance and editorial neutrality. The film opens with her recollection of starting to write and publish. The woman we see is older, the time is 1912, when she brought out her first collection, *Evening*. This and *Rosary* (1914) were both mainly about love. But the film is not about love. The year 1912 was also when her son Lev was born, and the film shifts immediately to the prisons, where her son spent many years. The point of the film is remembering, as in her powerful *Requiem* (1935–1940), about those who were caught up in the purges of the late 1930s. In her *Poem without a Hero, A Triptych* (1940–1962), remembering becomes a demonic thirst.

The film takes place in St. Petersburg, which she calls that "granite city of fame and calamity," "my squandered inheritance," "a useless appendage," "swinging from its prisons," her home. (See "Petersburg," 1914, *The Complete Poems of Anna Akhmatova*, Judith Hemschmeyer, trans., Roberta Reeder, ed. [Somerville, Mass., 1990], 1: 287; "Reed," 1929, *Complete Poems*, 2-A: 77; and "Prologue," 1935, in "Requiem," *Complete Poems*, 2-A: 99.) However, the repeated shot of the procession of mourners by her bier heightens a sense that the city was less her home than was the community of writers who lived there. "I lost my habit of being settled in one place." In most of the film's action, she ambles along, talking, behind the camera as it moves at a slow pace. This film-walk leads across a half-century of sorrow: the execution of her first husband, the poet Nikolai Gumilev, the imprisonment and reimprisonment of her son, the arrest and death of Nikolai Punin, with whom she lived, the arrest and death of Osip Mandelshtam, the poet, and her expulsion from the Writers' Union in 1946. At one point, Akhmatova despaired, "Why is our century worse than any other?" But Aranovich does not use this quote in the film. He carefully selects among her writings to portray not despair but deadly reckoning. "I will remember," she writes,

And if they gag my exhausted mouth through which a hundred million scream,
Then may the people remember me on the eve of my remembrance day."

"Epilogue II," 1940, *Complete Poems*, 2-A: 113.

Aranovich knows his audience. His film about Anna "of All the Russias" is a highly relevant work about the painful workings of memory.

The film begins with an encounter poem. Aranovich opens with the "you" of the postrevolutionary period, her Muse:

I said to her: 'Was it you who dictated to Dante
The pages of *The Inferno*?' She replied: "It was I."

"The Muse," 1924, *Complete Poems*, 2-A: 75.

The "you" in the 1930s became a "blind, red wall," Stalin, shown laughing and smoothing his mustache. The "you" became women waiting for news from prison. "I wove a wide mantle for

them from their meager, overheard words" ("Epilogue II," 1940, in "Requiem," *Complete Poems*, 2-A: 113). Larger still, the "you" was those imprisoned in the camps.

"You" was the Russian language, invoked against the Nazi invaders in 1942 in a poem to "Russian speech, Mighty Russian word!" ("Courage," 1942, in "Seventh Book" [1936–1964], *Complete Poems*, 2-A: 185). This "you" went from writer to writer. Akhmatova's famous meeting with the poet Marina Tsvetaeva in 1941 is conveyed by a few moments of a phone call. Akhmatova is calling. "I'm listening," Tsvetaeva says. Akhmatova says softly, "Yes, yes. That's the way it is. She is listening to me." Aleksandr Solzhenitsyn, "indescribable," was rendered in dialogue. "Do you know that in a month's time, you will be a most famous man?" "I know, but that won't last long." "He's a great writer," she comments, "giving us back our native language." Aranovich embraced the "you" to mark off Akhmatova's important biographical transitions and transitions in the history of the entire community. Her first husband, Gumilev, was dead. The camera moved along a trail. "They shot him near Berngardovka, along the Irininskaya Road . . . I found out nine years later and went there. Groves, a small curved pine . . . Pits. Two fraternal pits for 60 people" (quoted in full in R. Reeder, "Mirrors and Masks: The Life and Poetic Works of Anna Akhmatova," *Complete Poems*, 1: 87). With Boris Pasternak, the sketch is a snapshot, capturing the event of his expulsion from the Writer's Union. "I adore that man . . . one of the greatest poets of the twentieth century." The camera moves from the train station at Peredelkino, the writers' village, to his door. "Is Boris Leonidovich here?" her voice asks. His photograph appears on screen, a face looking intently into the camera. "Expelled?" he asked. "I nodded," she says.

As for other Russians, "the silent flow," the film raises some important questions. Crowds, depicted at her funeral, are panned by the camera, which slightly rocks the heads as if unable to pin them down. At one point, her driver repeats *Pravda's* view of Pasternak. "There's this writer, Paster, I think his name is." "A pig will never do what Pasternak did." Her voice is sad and confused. "What haters of people we are." Osip Mandelshtam's words appear, "We don't feel the country behind us." In conveying crowds of Russians, the film does not seem to be clearly asserting, "this is history." It is establishing mainly the voice of the poet. There are shots of workers tearing down statues after the revolution, workers in factories, workers in the camps, workers wheeling and marching, children cycling on a spring day, and pedestrians bustling on the streets while listening to a loudspeaker announce the verdict of a trial. These images convey confusion and hatred of pretense and of the trivialization of individual life—her emotions, not totalizing truth. The most compelling images are evoked by dancers to Stalin's tune, which is a light background for street scenes in the 1930s. Aranovich repeats the one where men and women, dressed in white, were dancing to a merry tune, their legs swinging from side to side, but there was no merriment on their faces. If an interpretive statement were to be generated by these shots of ordinary Russians, it would be that Akhmatova is not entirely sure how Soviet society had been constructed out of the Russian people. "A real hunt is on for the soul of an ordinary man," Akhmatova remarks.

Visually, this is a highly dramatic film; intellectually, it is subtle and satisfying. For the period of the 1950s, it heightens a sense of futility. "The justice that triumphs after many years is not that justice that your heart aspired for then." "A new epoch began. We lived up to it." It shows lost time—"For thirteen years [living with Punin], I had not been writing any poetry, thirteen years"—and the ravages of time. Her room is "a site of neglect and ruin." "The houses I lived in no longer exist." "My last tie with the sea is broken." This film is a documentary of enormous appeal to the current generation, which looks back at the Soviet era with a sense of separation, deep sadness, and frustration.

Jill Janows' film *The Akhmatova Story* is biography of a different kind. Its appeal rests in exotic history. Janows, writer, director, and producer, executive producer for cultural affairs at WGBH, Boston, makes her historical setting a political landscape, a society undergoing war, revolution, and repression. Janows does not step back from the question, "Why is our century worse than any other?" In explanation, the biographical matrix builds: World War I, "We grew a hundred years older, and it happened in an hour"; Osip Mandelshtam's death, "Night is coming on, and it has no hope of dawn"; Khrushchev's return of the exiles, "two Russias would

be staring each other in the face, one that had put it in prison, and one it had put there." This film displays Akhmatova's poetry as one source of her greatness, of equal weight with her courage and her attempt to preserve old Russian culture. "Words float up from somewhere, words uttered a half century ago."

Interviews with Joseph Brodsky and Anatoly Naiman, among others, subtly convey something different. The interviews focus squarely on Akhmatova's imagination, which Isaiah Berlin calls "almost a second sight" ("Anna Akhmatova: A Memoir," in *Complete Poems*, 2: 28). Brodsky explains the importance of poetry in Russian culture: "the business of Russia is not business. It is a logocentric culture. Poetry occupies a central position in society." The power of words, rather than the harshness of Russian conditions, is what moved Berlin in their famous meeting in 1945. He refers to her "marvelous account of her childhood by the Black Sea," "her marriages to Gumilyov and Shileiko and Punin, her relationships with the companions of her youth, and of St. Petersburg before the First World War. It is in the light of this alone that the succession of images and symbols, the play of disguises, the entire *bal masqué* of the *Poem without a Hero*, with its echoes of *Don Giovanni* and the *commedia dell'arte*, can be understood" (Berlin, 33).

What is best about this film are these interviews and the documentary footage, particularly for the early period. Janows might have brilliantly succeeded by allocating less space to her own somewhat limited interpretation and more space to Brodsky and Naiman. Aranovich, poet of the silver screen, matched the power of image to the power of words; Janows' film is more a classic dramatic narrative.

Carol S. Leonard

Russian Research Center, Harvard University

Stalin. Produced by Mark Carliner; directed by Ivan Passer. 1992; color; 2 hours, 46 minutes. Distributor: Home Box Office.

"It is a portrait, not a landscape," said producer Mark Carliner of his *Stalin*, which had recently appeared on the cable television network HBO. He was speaking at a special showing of the nearly 3-hour-long film biography and, I thought, implicitly responding to newspaper critics who had complained of what they thought to be its cramped and excessively intimate focus. The critics were disappointed at being deprived of scenes with masses of extras showing the panorama of revolution and war. Instead, the film uses crowd scenes from classic Soviet films, such as Sergei Eisenstein's *October*, or from the newsreels of Dziga Vertov: ropes pulling down the statue of the tsar, forests of scythes, the gate to Senate Square swinging open to admit the revolutionary masses, people scattering before machine-gun fire. Perhaps the critics found this to be an unnecessary obeisance to the standard propaganda images of Soviet cinema, and they lodged their complaints in the spirit of Aleksandr Solzhenitsyn's Prisoner X-123 in *One Day in the Life of Ivan Denisovich* (1963): "it's so arty there's no art left in it." Yet these are the cinematic images with which the Russians conjure up their revolutionary past, and it seems fitting both to render them this homage and to situate a biography of Joseph Stalin before their backdrop of artifacts.

Most of the action in Carliner's *Stalin* does occur indoors. Stalin, after all, was never known as one who had to be on the spot. During the war, he contented himself with newsreels and written reports, visiting the front only on a very few occasions and never addressing the troops, nor did he ever make spontaneous visits to factories. So it makes a certain sense that his film biography should unfold within the walls of the Kremlin. In fact, Carliner managed to shoot almost all the important interior scenes in the actual locales. Maximilian Schell, as V. I. Lenin, confronts Stalin about the Georgian nationalities question in the apartment actually used by Lenin. Robert Duvall's Stalin dies in the very room of the dacha at Kuntsevo where the dictator finally succumbed to the physical laws.

Carliner and director Ivan Passer strove for the most authentic depiction possible of the important scenes in Stalin's life. For this, they consulted a representative sample of Stalin biographers, including Robert Conquest, Robert C. Tucker, and Dmitri Volkogonov. Paul Monash's script attempts to reproduce many episodes exactly as they appear in historical accounts, often with dialogue from Nikita Khrushchev's memoirs or those of Svetlana Allilueva, Stalin's daughter. Sometimes, Monash seems to sum things up perfectly, as when Leon Trotsky, Grigorii Zinov'ev, N. I. Bukharin, and the other leading Bolsheviks shrug off the post of party general secretary in 1922, with Trotsky suggesting that the Politburo "saddle a mule for the job; saddle Stalin." And Stalin later remarking to his supporter Sergo Ordzhonikidze: "Imagine that—Trotsky, King of the Jews, turns the job down."

Within the limits implied by commercial film, the story has never been told with so much painstaking attention to detail and such dedication to accuracy, in a narrative that takes up almost four decades.

Those limits do cast their shadow over the enterprise. They dictate short, one or two-minute scenes with clipped, content-loaded dialogue. This is a portrait rather than a landscape. The elaborate rhetoric and the turns of the political struggle that permitted Stalin to rise to the top of the heap after the death of Lenin cannot find a place. Cinema intrudes into the historical interpretation and favors a purely bureaucratic story of his rise to power, seeing him as a no-nonsense administrator who pulled the organizational wires while his rivals, basically café types, chattered on about this or that question of theory. Actually, in *Stalin*, there is not even enough screen time to make Stalin's rivals out to be idle chatterers.

As a result, the portrait is of a man without much of a political history, less a product of the Bolshevik underground and the rigors of the revolution and civil war, less a devotee of Lenin and veteran of the tangles of exile factional strife, than a piece of refuse tossed up from the world of the Georgian village back streets, a mixture of "oriental" cruelty and criminal guile. Robert Duvall studied his character carefully, in order to affect a Georgian, rather than a Russian accent, while trying "to make a human being out of a dark, dark, dark guy like him." He concluded that Stalin could only be compared to Al Capone. This view has had its adherents, among them Trotsky himself, who thought of Stalin as a "limited, wily boor" and whose own biography of Stalin stressed his Georgian, even Ossetian, qualities, despite the author's fear of "venturing too far into the unprofitable region of national metaphysics."

The personal qualities most exploitable by film are those that stress Stalin's role as an alien interloper in the Bolshevik movement, and even an interloper in the national sense, a Caucasian scourge of the Great Russians. Lost in the process is the rest of the historical Stalin, whose life's quest, it seems clear, was not simply to rise to the power of an Ottoman despot, which he did, but to be admired as a politician and intellectual leader the way Lenin was admired, that is, as a man who, Anatolii Lunacharskii once said, "could see ten feet into the ground." While another tyrant might have been satisfied to be flattered as the Leader and Teacher of Mankind, the Friend of Children, the Master of Abrupt Turns and Unexpected Maneuvers, the Coryphaeus of Economic Science, to have his image projected on the clouds at rallies and woven into rugs in Turkmenia, Stalin had to have more. Bright enough to know that he could never be regarded as anything but a bumpkin in the world of the old Bolshevik intellectuals, he could not enjoy their flattery and did not, in the end, trust them with their lives. Emerging as a leader in an atmosphere of civil war, he found he could only prove himself in an atmosphere of permanent civil war. All this to be The Lenin of Today. Stalin's contradiction was not in the meanness of his background and gifts but in the contrast between these and his sublime ambitions.

The film sets Stalin the Ossetian interloper in bold relief from European types like Zinov'ev, Lev Kamenev, and Trotsky (who, amazingly, are presented as more or less continuous allies) and, in keeping with the conventions of the glasnost period in which the film was shot, from the saintly and truly Russian Bukharin. Bukharin's bitter fight against Zinov'ev, and its role in building Stalin's power, does not appear. He seems to be an innocent bystander. At the mysterious death of Leningrad party chief Sergei Kirov in 1934, the act that unleashed the

great purges, Bukharin complains about a "Reichstag fire" provocation. Not so. In fact, at the time, he accused the "fascists" Zinov'ev and Kamenev of murdering Kirov and called them the "Charlotte Cordays of the Russian revolution"—essentially the same charges for which they were shot two years later. But the myth of the genuinely Russian and humanist Bukharin as the alternative to Stalinism was the general line of the glasnost period.

Stalin has its limits, because of its particular form and the circumstances of its making. Perhaps a more historical Stalin film biography will emerge one day from a more theatrical script, a film of three or four hours' screening time. Until then, however, this one will have to be considered the best visual history yet.

Anthony D'Agostino

San Francisco State University

Waterland. Produced by Katy McGuinness and Patrick Cassavettes; directed by Stephen Gyllenhaal. 1992; color; 95 minutes. English. Distributor: Fine Line Features.

Although the central narrative of *Waterland* unfolds in England in 1943, this is not a film about World War II. The story it tells is an intensely personal tragedy that so consumes the principal characters that they, and we, scarcely notice the war raging around them. But this intimate tragedy is framed by a contemporary narrative in which the protagonist of the 1940s drama searches for the meaning of History some thirty years later. As the adult Tom Crick (Jeremy Irons) tells the story of his wartime experience to his high school history students, he acquires a new understanding of the subject he has been lifelessly catechizing for twenty years. In the end, *Waterland* suggests that history or, as Tom Crick calls it, "telling stories," is one of the few transcendent means of attributing shape and purpose to life in a secular society.

Both of the film's plots, past and present, are set in motion by the adult Crick's wife, Mary (Sinead Cusack), who abruptly announces that God will soon give her the child she has been unable to bear for decades. Mary seems to us to be slipping into madness, but Tom describes it differently: "she is slipping away to where I most fear to follow . . . back into the past." Regardless of his fears, Tom does return to the past because Mary's announcement has set his memory in motion, and it inexorably draws him back to the English fens during World War II. This personal crisis becomes linked to the more abstract question of the meaning of History when one of Crick's students, Price (Ethan Hawke), impatiently interrupts a deadening classroom lecture on the French Revolution to demand justification. "What's the point?" he asks angrily, "The only thing interesting I see about history is that it's about to end."

Incapable of answering Price's challenge and, mercifully, unable to continue with his account of the revolution, Crick takes his students with him into his own past. There, they find Tom and Mary as passionate adolescent lovers and Tom's "simple" brother Dick, who is the object of their love and incomprehension. As the adult Tom and his students wander the fens of his youth, the lives of the three principals there unravel in a tragedy that climaxes with Dick's suicide and Mary's harrowing abortion by an old fenswoman.

We, as viewers, can easily understand the high school students' delight as they visit Crick's rich past, which stands in marked contrast to the curricular History that he retails in the classroom. When Crick speaks of the fens, they spring to life for us and students alike: great expanses of grassy marsh stretch away toward pale blue sky on the horizon, and the shining, hopeful faces of young Tom and Mary come sharply into focus. In the film's most imaginative scenes, Tom so evocatively describes his family's history that he literally transports his pupils: we see them motoring down a English road to gaze upon people long dead and to visit a village that is celebrating the 1911 coronation of George V. Meanwhile, Crick's formal history lectures

are the antithesis of these vibrant and informal voyages. Trapped inside the classroom, we cannot even see the slides that he listlessly projects for the students. Listening to his flat naming of images—"the Bastille . . . Marie-Antoinette . . . the Tuileries Palace . . ."—we understand Price's question all too well. How can this possibly matter?

Crick cannot initially answer Price's question, nor can he give life to an account of the French Revolution, because he has denied the significance of his own history. Until the point at which the film begins, history seems to have been nothing more for him than a collection of facts that have neither emotive power nor lived meaning. Only by recovering his own past can he find meaning in History.

In the end, the "how" of Crick's history proves to be astonishingly simple: eschewing analysis, he just "tells stories," implying that meaning is inherent in even the most subjective narrative. But his "why" of History proves to be much grander. Nearing the end of his personal odyssey, Crick finally becomes able to answer Price. Having earlier suggested that "history teaches us about life and how to live it," he goes further in a final speech before his forced retirement. Recalling his experience in postwar Germany, as he helped dig bodies out of the rubble, he explains that "the only way to cope was to think of it as history, as part of a story, and not just as bits of meat." As he sees it, historical narratives—whether personal accounts or broader surveys—are transcendent, linking generations together and enabling human beings to make sense of their personal and collective lives. That such a project acquires special importance in a secular society is underscored midway through the film when Tom Crick's first attempt to justify the teaching of history, to the school principal, is followed immediately by Mary Crick's confrontational observation that the high school assembly no longer has a place for "the teachings of our savior, Jesus Christ."

Sadly, *Waterland*, which develops such a generous and humane interpretation of the importance of history, also implies that History is men's business. In its representations of Crick's relationships with his students, with Mary, and even in his memories of his father, the film repeatedly suggests that women are incapable of grasping the importance of history and using its transcendence to make sense of life. Price's female counterpart in the classroom is Judy, who tries to smooth over the discomfort that his challenge causes by claiming, "I like history. I don't care if it's got no point." Later, Judy and a female friend interrupt Crick's stories to complain of his repeated emphasis on his sexual relationship with Mary; not the least bit disconcerted, Crick suggests that they do not really understand the larger points that he is making about adolescent curiosity. And, in the end, it is the female students' concern about the sexual content of these stories that will be the teacher's undoing. Once Crick has been forcibly retired, Price explains that it is because some of the "girls" complained to the principal that his stories were disgusting.

If high school girls do not understand the meaning of History, Mary's status is far worse: she lives wholly outside of history in a realm of revelation and near hysteria. Tom and Mary Crick represent the two seemingly opposite poles of reason and emotion: while Tom is in the process of retrieving and making sense of his own past, and so recovering History, Mary prepares to steal an infant from a supermarket. She later explains her actions with the justification that "I knew no other way to break out of this prison [of unhappiness]." Finally, as if to underscore just how far outside of history Mary is, Tom tells us that, when she left him, her last note asked that he return her library book—which is two weeks overdue.

It would be nice to believe that *Waterland* is simply representing the gendered world that is one of the sources of Tom Crick's trouble, for his overly rational approach brings him to his original impasse. But this is a barely tenable interpretation. For while Tom Crick is permitted to confront and assimilate the painful emotional lessons of his past, Mary can only steal babies and run away. By so markedly gendering the world and, more important, by so markedly gendering who may "tell stories" and know their full meaning, this beautiful and poignant film threatens finally to re-create the problem that it first set out to solve.

Laura Mason

University of Georgia

Outremer [Overseas]. Produced by Serge Cohen-Solal for Paradise Productions; directed by Brigitte Rouan. 1990; color; 98 minutes. Distributor: Aries Films.

Since the appearance of Henry Rousso's *Le syndrome de Vichy, 1944–198..* (1987), the study of collective memory has become a leading sector of French historiography. A representation of the French experience in Algeria, Brigitte Rouan's *Outremer* provides such research with a site where the public memory of that experience was further shaped. The next step would be to place the film, its box office, and the debate evoked by its screening and by interviews with its makers alongside similar evidence connected with other recent films dealing with the Algerian war such as *Cher frangin* (1989) and the British-made *La guerre d'Algérie* (1990), which the *pied-noir* group *Recours* attempted to block from French screens. Benjamin Stora, the leading historian of the memory of the Algerian war, includes *Outremer* in his chronology of the most recent phase of that phenomenon but does not comment on it further. (See *La gangrène et l'oubli: La mémoire de la guerre d'Algérie* [Paris, 1991], 364.)

The film centers on three sisters (Zon, Malène, and Gritte), members of a wealthy *colon* family. Except for the final scene, the action takes place in Algeria between 1946 and independence. *Outremer* covers the same key incidents in the family's life three times, focusing each time on the perceptions of a different sister and thus revealing in subsequent versions new elements that yield a different understanding of the events.

As a contribution to the legacy of the French-Algerian connection, the film can be seen as a evocation of *pied-noir* nostalgia. Rouan uses considerable skill at cinematic *mise en scène* to offer the viewer the sensual feast that Algeria presented, especially during the *belle époque* before the outbreak of the nationalist revolution in 1954. For Rouan, this was "the luminous, joyful background of my childhood, festive, carefree, filled with smells . . . completely explosive, blinding with light, fruit, and flowers" (from an interview with Rouan distributed in North America by Aries Films as part of the publicity for the film). It is in this setting that we are introduced to the sisters, their adventurous energy, and their search for the "Prince Charming" who will organize their love and their lives. Their men appear first in brightly lit up-angle shots, one even given a spotlight halo while sitting on a ship's sunny deck. Rouan's use of the "Triumph of Love" finale from Gluck's *Orphée et Eurydice* to frame each sister's segment enhances the film's romantic theme.

At the same time, *Outremer* does not indulge that nostalgia with a rosy lens. Never departing from its focus on the *colon* viewpoint, it shows the increasing intrusiveness of the "events" that were "spoiling our fun." Rouan's characters try to ignore the war at first, just as they have all but ignored the presence of the Arab population. No Arab is given a personality or more than a few lines: Arabs are the quintessential Other. When arson occurs on Malène's farm, suspicion falls on the laborer Lakhdar, born and raised on the *colon*'s farm. The steward at first rejects the suspicion: "Lakhdar is like my son." But within a few seconds, he changes his mind and begins to beat him wildly. The racism of the *colons* emerges with special force in a savage family dinner conversation: Arabs have no guts; they should all be drowned in a bottomless boat.

Even as it portrays the *colons'* bigotry, however, *Outremer* makes a parallel, exculpatory argument. In an interview about the film, Rouan noted the "injustice" of the *colons'* actions, but she argued that all the characters were "victims of their roles, of preconceived ideas transmitted to them by tradition." Certainly, the film emphasizes their acceptance of the reigning notions of gender and their loyalties to their family, to their *colon* class, and to France. Zon, the oldest sister, remains encased in a tight domestic circle of naval officer-husband, children, servants, other family, and Catholic faith. She boasts to her sisters of being "united in love" with her mostly absent husband Paul, her Prince Charming. Malène, the middle sister, marries a passive type who is thrust unwillingly into his role as a farm owner and always filmed reading a book. Forced to become the real operating manager of the farm, she protests to him how much she hates wearing the pants. She hates always looking down at his seated figure: "I want to look up

to you." Gritte, the youngest, engaged to Paul's vacuous brother Max, cannot make up her mind to marry him: she is waiting for a Prince Charming "to carry me off on a white horse."

The ties of family and class are difficult to escape. The sisters' father still dominates. Gritte, a nurse, complains that he would not let her be a doctor, which would have given her a way to leave Algeria. It is still the men who do the talking at the classic site of domination, the dinner table. At the same time, the women help to reinforce attitudes the script attributes to their class. While reciting her catechism, Zon's daughter asks if the Arabs, too, are our brothers. Zon is caught short: "No, yes, well, only in Christ." Malène, complaining that the sun on her skin is making it wrinkled "like an Arab squaw," finally cracks under the strain of managing the farm and sets fire to the granary. She then allows Lakhdar to take the blame. Terrified by a sniper attack on the farm, she nevertheless proclaims that, because they have been attacked, it would be wrong to leave. Gritte, the most complicated and ambivalent of the sisters, willingly joins them in a patriotic *colon* ditty, "Nous les Africains." Hearing DeGaulle's 1958 *je vous ai compris* speech, which *colons* took as a promise to maintain a French Algeria, she cries, "We're saved!" to which her fiancé replies that DeGaulle has become her Prince Charming. That the pressures of class ideology, prescribed social roles, and family solidarities appear to define choices and to overwhelm the desire of some characters to run against the tide of adherence to *Algérie française* may help explain why the film did not produce the same intensity of *pied-noir* protest encountered by *La guerre d'Algérie*.

As a primary source for public memory in the 1990s and a visual memoir by a film director who spent her childhood in Algeria and her adult life in Paris, *Outremer* raises two sets of questions. The first concerns the response in contemporary France to its arguments about such matters as gender, romance, racism, victimization, and individual responsibility. Does the specificity and exoticism of a past colonial situation obscure the application of the film's arguments to contemporary France, or do most viewers perceive the title's irony: the truths observed obtain not just *outremer* but here in the *hexagone*? The second concerns the ways in which Rouan's vision of Algeria, a mixture of personal memories, later learning, and imagination, opens up questions about the French experience there. How, for example, did the structure of the colonial situation limit the range and complexity of acceptable sex roles? Did the obsession with "Prince Charming" arise from the intensification of aristocratic-military values in the *colons'* elite stratum? Did the domination of Arabs inherent in the Algerian context demand dominating males as the only acceptable types despite—as in the case of Malène's husband—their lack of the requisite qualities? Did the *pieds-noirs* who settled in France thus introduce to the Metropole an especially limiting notion of gender? What role did women play in the *colons'* near-universal refusal to consider an alternative to a French Algeria? The history of the final decades of French Algeria thus awaits research indifferent to the boundaries of "private" and "public" sphere, sensitive to the connections of gender and politics, and willing to investigate fully the relations between racism, class, and leadership.

John H. Weiss

Cornell University

La plaza del Diamante [Diamond Plaza]. Produced by Figaró Films; directed by Francesc Betriu. 1982; color; 110 minutes. Distributor: Facets Video, Chicago.

Los santos inocentes [Holy Innocents]. Produced by Ganash P.C.; directed by Mario Camus. 1984; color; 107 minutes. Distributor: Facets Video.

La mitad del cielo [Half of Heaven]. Produced by Luis Megino P.C.; directed by Manuel Gutiérrez Aragón. 1986; color; 124 minutes. Distributor: Facets Video.

These three films offer a stimulating approach for understanding the contemporary history of Spain. An adaptation of the well-known novel by Mercè Rodoreda, *Diamond Plaza* is among the most significant movies produced since the democratization of the country, powerfully reflecting the idiosyncracies of the Catalan character and historical circumstances of the twentieth century. The film impressively evokes the dictatorship of Primo de Rivera (1923–1930), the Second Republic and Civil War (1931–1939), and the first years after the war—the darkest period of Franco’s regime—in the town of Gracia, today an important working-class district of Barcelona. The historical frame of the film goes beyond socio-political events, however, to focus on the internal dramas of the common people during this long period and conduct a sharp analysis of the attitudes of this class as well as of the condition of working women.

A Catalan with anarchist beliefs, Francesc Betriu employs an austere style whose limited aesthetic (the film is a reduced version of a television series) is perfect for the story written by Rodoreda, a best-seller translated into numerous languages and considered one of the most important works in modern Catalan literature. The novel was typical of Rodoreda, beautifully written with a polished style, full of images and formal complexity, but melancholy and offering little hope. In fact, Rodoreda’s personality and the spirit of the original text “eat away” at the film, so much so that the film’s narrative seems imprisoned by the literature; at times, the movie succumbs to the novel and loses filmic orientation, even to the extent of employing a voice-over reading of passages from the text. Nonetheless, *Diamond Plaza* has some very successful sequences: the dance at the great tented Gracia town fair, the sentimental goodbye in Gaudí’s “Güell Park” when the lover marches away to the Aragon front, the leave-taking of the child in the refugee camp during the Civil War, the bombing of Barcelona, and the courtship of the petit-bourgeois Catalan storekeeper. And the great revelation of this film: the acting of Silvia Munt, whose role as the character of the “Colometa” is that of a simple dreamer typical of that era.

Betriu’s calm rhythm draws viewers into the drama. The movie’s lyricism and sense of humor is at one with the novel’s spirit, and the director uses ellipses—for example, “Colometa’s” disillusion with love on her wedding night—to counterbalance the tragedies of the protagonists and move the audience, without falling into a facile sentimentalism. Although *Diamond Plaza* is a minor work compared to the great historical dramas of European cinema, it does possess certain brilliant touches. It is also a homage to the political, linguistic, and cultural strength of the common Catalans, who had to put up with Franco’s dictatorship while dreaming of their frustrated nationalist awakening and awaiting liberty’s resurgence. Thus, like the novel, the film has great didactic value in explicating the idiosyncracies of Catalonia.

Holy Innocents received awards at the 1984 Cannes Film Festival for the acting of Francisco Rabal and Alfredo Landa. Based on the novel by Miguel Delibes, this film effectively portrays the attitudes of rural people in Extremadura during the 1960s. The book verged on being a great novel, in which the personality of the prestigious Spanish writer came through strongly.

This created problems for the film's director, Mario Camus, one of the leaders of the New Spanish cinema, who seemed unable to free himself from the literary tone of the text, although he knew how to "translate" it brilliantly. His success is remarkable, given the fact that he produced the film for Spanish Television, where he faced restrictions that were at the very least formal, since the film was made for broadcast on the small screen.

Camus was able to make a film that attracts intellectuals, who are moved to critical reflection by the work; however, it drew little enthusiasm from the general public. The crystalline images of *Holy Innocents* have a power much like the original novel, and the film's great plastic beauty is aided by the splendid natural scenery of Extremadura, as well as by the actors' capacity to re-create local types. Aside from the recognition given Rabal and Landa for their portrayals of "Azarías" and "Paco el Bajo," the work of Terele Pávez, Juan Diego, Agustín González, Mary Carrillo, Agata Lys, and Maribel Martín is worth noting. As Miguel Delibes stated: "Paco Rabal plays a subnormal person perfectly. In my judgment, this is a very difficult role, for you must be someone who can only communicate with birds and children. The look, the gestures of the hands, the way the arms move . . . one sees that one is observing a subnormal person. He didn't invent the character but adapted it once he had studied it carefully. Together with Rabel's stupendous acting is that of Alfredo Landa as Paco el Bajo, Iván's gunbearer, who is really a marvel. The expression of his eyes is like a poem; he says everything with his eyes."

In order to put Delibes's novel into images, Camus used eminently analytical montage to give great precision to this dramatic, even cruel, story, which nonetheless still contains a certain sense of humor.

Half of Heaven may well be seen as one of the most important films of the Spanish cinema during the democratic era, having been awarded the Grand Prize at the San Sebastián Film Festival in 1986. In this work, Manuel Gutiérrez Aragón returned to his creative sources, the dramatic and imaginative style that characterized his more notable films: *Habla, mudita* (1973) and *El corazón del bosque* (1978). The latter work focused on the *maquis*, while including the director's recurring interests: the difficulty of communication, his taste for magic elements, the opposition of countryside and city, and his fascination for the rural spaces of Cantabria, his native land. There, rooted in his memories and the study of *mentalité*, Gutiérrez Aragón is at his best, creating a master work.

Half of Heaven recounts the moral and existential itinerary of a young mother, "Rosa" (Angela Molina, enigmatic and majestic), who suffers the social and political misery of Madrid during the second postwar period and the beginning of Franco's decadence. Thus it evokes the Spain of the 1950s and 1960s, with a focus on daily life in the capital through both copious detail and a fantastic tone. Margarita Lozano plays the grandmother splendidly; her story of witchcraft is significant in the film, for her granddaughter—Rosa's daughter—"Olvido" inherits that practice from her. This microcosm of postwar Spain was prodigiously reconstructed in the studio. It is only right to give credit to the work of the artistic director, Gerardo Vera. Director Gutiérrez Aragón sought an evocation of a rich life gone by: "The Madrid of *Half of Heaven* is not a realistic Madrid. My first vision was given to me by a servant—up there in the north where I come from—who told stories about Madrid's luxurious houses, its 'ripper' killers, its dances, its pimps, ministers, and artists. My film is a story of phantoms. I think that all my films are stories of phantoms, even those phantoms that don't appear in images."

Nonetheless, the film also contains a critical background that takes it beyond the epoch it calls up, for Rosa is a social climber with the condemnation that contemporary values bring to that label. Gutiérrez Aragón explained this second reading: "Sometimes I ask myself if *Half of Heaven* is the story of an ambition. The story of someone who is ambitious is always dramatic and terrible, because an egotist never has enough. While shooting the film I noticed that I have described the history of today's Spanish socialist. I think it could be titled 'History of a socialist' or something like that."

The film has moments of great emotion and some marvelous sequences, for example the scenes in which Fernando Fernán Gómez appears—such as those in the market and the restaurant—or the dance of the two officials, in which Antonio Valero stars. Gutiérrez Aragón displays sensitivity and aesthetic taste in the internal conception of the story, although the

second half occasionally loses dramatic-narrative equilibrium. This affects the film's credibility, since the clean and synthetic first part is analytically elongated without finding the middle way between naturalism and fiction.

In sum, these three films made during the democratic period in Spain provide a relatively accurate image of Spanish contemporary history, above all that of the long Franco period and its immediate antecedents.

J. M. Caparrós-Lera

University of Barcelona

LATIN AMERICA

Barbosa. Directed by Jorge Furtado and Ana Luiza Azevedo. 1988; color and black & white; 13 minutes. Portuguese with English subtitles. Distributed by the American Federation of Arts, 41 East 65th Street, New York, N.Y. 10021 (212) 988-7700, as part of its "Short Films from Latin America" series, Program Five: Heroes and Healers.

Game day in Rio de Janeiro defines Brazilian national identity. Fear, fortune, fate, and fame swirl around everyone and everything. Even riding the bus to Maracanã Stadium, the world's largest, is emotional and overwhelming. Fans, gaily dressed in their team colors, wave their team flags out the windows. The bus reverberates as team songs find their accompanying beat in headrest thumping and window banging. Riders not going to the game sit by idly, some feeling embarrassed but most caught up in the ebb and flow of collective understanding.

Jorges Furtado and Ana Luiza Azevedo's *Barbosa* plays on and captures the all-encompassing nature of soccer in Brazil. Indeed, *Barbosa* is about deep anthropological meaning, as Clifford Geertz might say if he were on that bus. For Brazilians, soccer is not just a game and Brazil is not just a country. Soccer is the Brazilian psyche.

Barbosa is based on Paulo Perigão's *Anatomia de uma derrota* (Anatomy of a Defeat). It revolves around the attempt of an unnamed character (played superbly as dour and depressed by Antonio Fagundes, one of Brazil's finest actors) to travel back in time and change the outcome of the 1950 World Cup Soccer Championship, when a last-minute Uruguayan shot slipped by goaltender Moacyr Barbosa. The result was more than a loss. Brazil was plunged into national disgrace, and Brazilians of that generation remember the defeat in the same way that many U.S. citizens remember the assassination of John F. Kennedy.

Barbosa might be called a self-conscious "docu-fantasy." The montage intersperses newsreel and acted footage in order to let the main character travel from 1988 back to 1950 with ease. This is not, however, a simple time-travel film. It plays on viewers' assumptions about history and historical films by mixing acted and actual newsreels and by ignoring chronology to blur the line between past and present. The final scene takes the filmic trick to an even higher degree when the actor sits alone in Maracanã in 1950 as an interview in 1988 with the real Barbosa is beamed into the character's modern laboratory. Yet *Barbosa*'s goal is not entertaining fantasy or cinematic pretension. The film is about the Brazilian sense of continuous disillusion. The past cannot be changed, and Fagundes' character cannot modify the outcome of the 1950 World Cup. The message is clear: present actions cannot change historical injustices.

The films begins as a straight documentary. Footage of the 1950 Brazilian National Team

is accompanied by the official national victory song, which neatly combines notions of salvation, Brazilian nationalism, and the building of the stadium. Rio de Janeiro's mayor announces to the 200,000-strong crowd: "Brazilians . . . You have no rivals on this planet . . . I have kept my promise, building this stadium. Do your duty: Win the World Cup." Gradually, the newsreel footage that dominates the screen shifts. The past becomes part of the present as a middle-aged man sadly sits in front of his television watching the same footage we have just left. The image of Uruguay's winning goal is replayed over and over in his mind as he remembers being there as an eleven-year-old, when "I [was] certain that all my dreams were possible. [When] a world that once appeared trustworthy and submissive revealed itself as uncertain and absurd."

Barbosa does more than confound reality and fiction through its visual syntax. The language in the film does the same. Fagundes' character does not remember the game as it was, he remembers it as an image in "aggressive black and white." For race-conscious Brazilians, the "whiteness" of Uruguay's team no doubt contrasted markedly with the "blackness" of their own. The defeat was more that a humiliation of geographic nationalism, it reinforced the cultural inferiority so many Brazilians felt (and feel) with regard to their southern neighbors. As Fagundes watches a 1988 television interview with the real Moacyr Barbosa, who complains, "We can't return to the past," Fagundes decides to do just that. A time machine returns him to the 1950 game, and he plans to change his own destiny, along with Barbosa's and Brazil's. Again, factual and fictitious images are turned on their heads as Fagundes brings his video camera along, allowing him to see the past as both a participant and an observer.

July 16, 1950. World Cup Day. Rio de Janeiro seems a happy place. The streets are clean and uncrowded. Yet the reality is different. "200,000 people are on their way to a tragedy . . . as [proof] that nothing will go right in this country." At Maracanã Stadium, Fagundes pulls up in a taxi, but "those 200,000 people didn't know God had just arrived. At that moment I was greater than history." Fagundes (*Fagundeus* [God]?) is met by a man dressed in white (an angel? his father? both?), and the two, and the video camera, are ignored. We see God and his assistant in the same colors that they see when viewing reality directly through their eyes. Yet the game is watched through the video camera, and the slight distortion has a huge repercussion. The game is only in black and white; it must remain as the immutable past. Fagundes hurries to the field, and when he does so the black-and-white past becomes real (in color). Yet Fagundes cannot change history. His scream "Barr—bosss—a" seems to distract the goalie and cause him to allow Uruguay's winning goal. Has God abandoned Brazil even when given a second chance?

In the final scene, a color newsreel of a solitary Fagundes, alone in the empty Maracanã Stadium, appears on his own television again in 1988. The credits roll by. Yet the hope that springs eternal in Brazil, "o país do futuro e sempre será" (the country of the future, and it always will be), remains. The parting shot comes from the real Barbosa, who laughingly claims, "To be an artist is difficult."

In barely thirteen minutes, Furtado and Azevedo's *Barbosa* illustrates Brazilian national identity and reflects Brazilian national consciousness. As a historical film, it is especially potent since it represents the past even while its images ask, "What is history?" *Barbosa* also creates a "Brazilian mood." Soccer, disillusion, and hope become the same yet remain oddly different. The past is unchangeable. The present and the past are one. And God remains a true Brazilian.

Jeffrey Lesser

Connecticut College

NORTH AMERICA

Margaret Sanger: A Public Nuisance. Produced by Barbara Abrash; directed by Terese Svoboda and Steve Bull. 1992; color; 27 minutes. Distributor (video): Women Make Movies, 225 Lafayette St., Suite 212, New York, N.Y. 10012 (212) 925-0606.

The topic of reproduction occupies a formidable place today in both the academy and the public mind. While the women's movement and recent feminist scholarship can be largely credited with this development, attempts to thrust reproduction policies and practices into the center of public debate go back much further. One of the earliest figures in the United States to do so is now the subject of a 27-minute historical documentary titled *Margaret Sanger: A Public Nuisance*. This imaginative film traces Sanger's efforts to publicize the cause of birth control in the United States during the early twentieth century. It tells the story of a woman whose direct-action tactics made her "a public nuisance" but forced American society to concede defeat in keeping birth control a private matter.

A Public Nuisance uses a dazzling array of visual material and a compelling first-person narration in its quest to make the story of a direct-action forebear immediate and relevant. Reenactments, archival footage, drawings, newspaper clippings, photos, and graphics are all used to represent the political drama of Sanger's career between 1913 and 1923. In particular, these visual materials are used to emphasize the aspects of Sanger's life that reflect the iconography of the political activist: her conscious flouting of laws with which she disagreed, her distribution of literature and even a film protesting the Comstock laws, and her incarceration and public trial. *A Public Nuisance* focuses on conveying the relevance of Sanger to contemporary concerns. For example, Sanger's political ideals are represented as a series of slogans (birth control = women's health, birth control = economic well-being, birth control = sexual equality). These slogans are reminiscent of the gay activist slogan "silence = death" and thus connect Margaret Sanger to contemporary activism.

Many of the techniques of representation used in *A Public Nuisance* challenge basic historical conventions. Unlike most written works of history, which exhibit both a reverence for authenticity and a reluctance to call attention to the narrative inventions they rely on, *A Public Nuisance* unabashedly and sometimes quite self-consciously uses artifice and illusion to represent the past. Reenactments are interspersed with archival footage. In addition, the film includes slapstick skits that are neither reenactments nor drawn from archival material. Rather, these vaudevillian scenes are self-consciously dramatic and echo the larger themes of the documentary but are not directly part of the Margaret Sanger historical record. *A Public Nuisance* also self-consciously uses genre. It is made in the style of a silent film. The delightful score composed by Miki Navazio and the ingenious use of old-fashioned titling cards create the atmosphere of a old movie, transporting viewers back in time.

Some historians may be troubled by the use of the "made up" skits and the abandon with which the film uses drama and artifice. Although there has been a growing enthusiasm for theories that problematize the distinctions of fact and fiction, art and history, and artifice and authenticity, they have had a modest impact on historical practice. *A Public Nuisance* in a sense calls the critic's bluff by shedding the conventions in practice. Whether one is comfortable with this approach perhaps depends on one's relationship to the new criticism, in both theory and practice.

If the strength of *A Public Nuisance* lies in its imaginative challenge to the conventions of historical narrative, its weaknesses lie in the historical and political depth it offers viewers. At times, *A Public Nuisance* seems to provide simple answers to complex questions. For example, the film explains Sanger's public championing of birth control by quoting her as saying, "I

found myself in the position of one who discovers that a house is on fire and it was up to me to shout out the warning." This "eureka moment" view of history in which a concerned individual is moved by a social injustice is indeed one that Sanger herself embraced. She attributed her activism (in her autobiographical writing) to her encounter with Sadie Sachs, an unfortunate slum-dweller who died in 1912 while trying to terminate an unwanted pregnancy. The film thus seems to mimic Sanger's own explanations of historical change. Sanger, like many muckraking social reformers of the era, drew upon a nineteenth-century discourse of sympathy to focus attention on new areas of social reform. Instead of interrogating this discourse and the Progressive era of which it was a part, the film seems merely to re-present it.

The underlying dramatic structure of *A Public Nuisance* also appears to work against a fuller exploration of the historical and political significance of Sanger's work. The historical drama in *A Public Nuisance* is conceived as a story of victory. It begins with images of defeat, a jailed Margaret Sanger, and ends with a symbol of victory, champagne flowing. While this dramatic structure undoubtedly simplifies the task of filmmaking, it also obscures the uneven and contradictory nature of social change. Success was not as clear-cut as the film's structure suggests. For example, the monopoly afforded doctors in prescribing birth control was a condition of its initial legality, and this has been considered by some to be a loss for democracy and for women in particular.

Not only was success more tentative than the film suggests, but the sources of that success may be more varied than acknowledged. Victory, viewers are told, resulted from radical political action taken by a woman who refused to conform. The film ends with Sanger saying, "I might have taken up a policy of safety, sanity, and conservatism, but would I have gotten a hearing?" This assessment will sound particularly awkward to historians aware of Sanger's increasing conservatism after World War I. Sanger does seem to have taken up a policy of safety and conservatism, and this new strategy had an important effect on the fortunes of the birth control movement. Personally and in her public work, Sanger abandoned many of her radical ideas and tactics. Instead of attacking capitalists, she enlisted the support of George Eastman and the Rockefeller family. Indeed, she married J. Noah H. Slee, president of the Three-in-One Oil Company, who became the largest donor to the cause of birth control. Ideologically, too, there was a shift. For example, in 1919, Sanger announced, drawing on the rhetoric of eugenics, "more children from the fit, less from the unfit—that is the chief issue of birth control" (David M. Kennedy, *Birth Control in America: The Career of Margaret Sanger* [New Haven, Conn., 1970], 115). Thus the question of Sanger's relationship to early twentieth-century discussions of science, motherhood, sexuality, and health with all their flaws and limitations is left unexplored.

Readers must be reminded, however, that *A Public Nuisance* is only 27 minutes long. During that time, important aspects of the Margaret Sanger story are covered, including the major events of Sanger's initial fight for birth control and the historical importance of her political style. In a society plagued by deep social injustice, the filmmakers' enthusiasm for a woman who drew on the language of social injustice to challenge the code of silence surrounding birth control will resonate with many.

Eva Moskowitz

Vanderbilt University

History and Memory. Produced and directed by Rea Tajiri. 1991; color and black & white; 33 minutes. Distributor: Women Make Movies, 225 Lafayette St., New York, N.Y. 10012 (212) 925-0606.

Memories from the Department of Amnesia. Produced and directed by Janice Tanaka. 1992; color and black & white; 13 minutes; **Who's Going to Pay for These Donuts Anyway?** Produced by I.T.V.S.; directed by Janice Tanaka. 1992; color; 54 minutes. Distributors: Electronic Arts Intermix, 536 Broadway, New York, N.Y. 10012 (212) 966-4605; National Asian American Telecommunications Association, 346 Ninth St., San Francisco, Calif. 94103 (415) 863-0814; Video Data Bank, 37 S. Wabash, Chicago, Ill. 60603 (312) 899-5172; V Tape, 183 Bathurst St., Toronto, M5T 2R7, Canada.

Days of Waiting. Produced and directed by Steven Okazaki. 1991; color; 28 minutes. Distributor: Mouchette Films, 22-D Hollywood Ave., Ho-Ho-Kus, N.J. 07423 (1-800) 343-5540.

Although social historians have reconstructed the experience of marginalized peoples absent until recent decades from the pages of history, "thick descriptions" based on the "documentary or objectivist model," to use Dominick LaCapra's phrase, are insufficient. Subjective dimensions of the historical experience obviously cannot be communicated by the "hard facts." Such is the case with the relocation of Japanese and Japanese Americans, most of them on the West coast, to concentration camps in remote and uninhabitable areas of the United States during World War II. Autobiographical accounts or oral history compilations like Yoshiko Uchida's *Desert Exile* (1982) and John Tateishi's *And Justice for All* (1984) exist but are too few. Fortunately, a number of imaginative independent film and video makers, whose work constitutes a rich intertextual narrative, are "writing with the camera" to create images that fill in the lacunae of a unique but tragic legacy.

Stereotyped as the "quiet Americans," Japanese-American artists are working against the grain of their cultural heritage to retrieve the history of parents and grandparents who have chosen to remain silent. As a result, their personal investigations not only parallel the work of traditional historians intent on exploring the past but also exemplify the recent documentary filmmaking practice of foregrounding the self-reflexivity of the medium. A significant component of these films, in other words, is an interrogation of the ways in which the past is constructed and reconstructed.

Particularly focused on the importance of images in public representations of history is Rea Tajiri's *History and Memory*, reviewed in this section two years ago. Tajiri's postmodernist concept of the past is essentially visual: she differentiates between events that have "happened" or have been "restaged" before a camera and events for which there is no visual record. Among the latter is a picture in her own mind of her mother filling a canteen on a hot day in the desert, an image that needs to be contextualized in relation to a narrative. Attempting to connect the picture with the story, Tajiri reconstructs her mother's journey as an internee at the Salinas Rodeo Ground Barracks in California and at Poston, Arizona, ironically, the site of a Native American reservation converted to house the relocated Japanese. The narration of the film articulates the basic dilemma of Japanese Americans: they are absent from history because the dominant discourse has marginalized them, and they have themselves remained silent about a

painful past. A reconstruction of disquieting images that Tajiri's mother "forgot to remember," *History and Memory* counters both political and personal repression. Yet the traces remain of a historical legacy that has dictated invisibility—a strategy that Japanese Americans adopted or were forced to adopt except when they were brought "clearly into view" by the events of war. Although Tajiri uses voice-overs of family members to comment on her montage, her family still constitute an absent presence because they remain off screen. As her mother's voice explains in justifying historical amnesia, "you'd . . . go out of your mind, so you just put those things out of mind." A fleeting shot of her shows a woman escaping the probing eye of the camera. A response to public history based on the montage of feature film and newsreel images that exists in our collective memory, Tajiri's representational strategy mirrors to some extent the dominant discourse it engages in debate. She intercuts her own photography of the remnants of tar paper shacks in Poston, for example, with clips from *Bad Day at Black Rock* (1955), a feature film in which Spencer Tracy searches in vain for a murdered Japanese-American farmer. Ultimately, the erasure of the subjectivity of Japanese internees succeeds in foregrounding discursive issues that appeal to postmodernist critics engaged in textual analyses rather than confronting head-on the disturbing moral issues in question.

As opposed to Tajiri's emphasis on historiography, Janice Tanaka chooses to investigate the past by focusing her camera unrelentingly on her parents, a deceased mother and a father declared insane. In *Memories from the Department of Amnesia*, a geometrically structured work, Tanaka uses avant-garde technique in the first half of the film, intercutting footage of a young man on a bicycle circling inside a diner with a blank screen in which a physician in a surgical gown advances and retreats; these two figures occupy each other's space as well as inhabit their own. The sound track consists of eerie, muffled, and indistinguishable noises. Just when the film appears to be inaccessible, music begins, and snapshots of Tanaka's mother, Yuriko (Lily), flicker momentarily on the screen to signify how irretrievable the past has become. The second half of the film, in black and white, bisects time so that the past is represented on the screen and the present becomes a commentary on the sound track. The additional use of a split screen results in yet another division. On the left, a succession of photo negatives slowly develops into prints, while on the right, the camera restlessly scrutinizes photographs reproduced on a larger scale. As brief texts inscribe the photos with biographical data that add up to enormous personal tragedy, the filmmaker and her daughter are heard on the sound track reminiscing with laughter about a woman who "seemed so timid and weak" yet exhibited a passion for dancing and fast cars. Exactly how history and biography intersect remains unexplored, but the data about Lily's divorced husband, arrested by the FBI in 1941 and declared insane in 1946, invite speculation.

Tanaka's unflinching and moving portrait of her father, *Who's Going to Pay for These Donuts Anyway?* shot in the interview format as well as in the style of *cinéma vérité*, squarely confronts the issue of history versus biography. The film begins with a shadowy image of her father on the screen—a figure who will eventually come into sharper focus—as Franklin D. Roosevelt's "day of infamy" speech is heard on the sound track. A work that is more dramatic and accessible if stylistically less inventive than *Memories from the Department of Amnesia*, this film represents a collaborative effort by the director and her two grown children to reunite a family torn apart by the internment experience. As such, it translates documentary filmmaking practice into an investigation of the intersection of family dynamics and history.

Shortly after her mother's death in 1988, Tanaka searches for her father, Jack Koto, and reestablishes contact with her uncle, William Togo, neither of whom she has seen since childhood. After locating Jack in a halfway house for the mentally ill in a skid row section of Los Angeles, Tanaka and her children record their struggle to come to terms with "what it meant to be related to this man." Tanaka states with honesty, "observing the effects of the past could only be dealt with behind the distancing lens of the camera." Yet the painful and bewildering process of renewing contact with a father who has been a victim of "psychological genocide" proves rewarding. A shadow of the man he once was or could have been, Jack nevertheless retains his intelligence and wit. During one of his escapes from a mental institution, for example, he went to work for a bakery and delivered leftover donuts to police, who were billed

at the end of each month. Tanaka wryly comments, "I would like to believe he was collecting redress long before it become a political issue."

By showing the effects of the internment on several generations of one family, *Who's Going to Pay for These Donuts Anyway?* dramatizes the history of visible racial groups whose assimilation will always remain problematic. A newscast proclaims in stentorian tones at the beginning of the film, "we cannot assimilate Japanese into our national life"—an expression of racism that was translated into governmental policy by Executive Order 9066. Significantly, Jack Koto and William Togo were brothers whose father was an impoverished samurai with a strong sense of pride and dignity. Angered by the racism of the "imperialist white man," the elder Tanaka refuses to seek American citizenship when it finally becomes an option after the war. But Togo, photographed in a cowboy outfit in childhood, chooses to assimilate, becomes a Christian, and remains pro-American despite arrest and detainment by the FBI and relocation to Manzanar. A UCLA graduate who wrote editorials for *Rafu Shimpō*/the *Los Angeles Japanese Daily News* before the war, Togo rebuilds his life, prospers, and wins numerous civic awards. By contrast, brother Jack is so enraged by his internment experience that he is unable to relate to his family and is diagnosed as schizophrenic with paranoid tendencies. *Rafu Shimpō*, described by Togo as "schizophrenic" because it was printed in both English and Japanese, serves as a metaphor for the contrasting experience of the two brothers. Years later, as the two estranged brothers reconcile before the camera, the filmmaker comments on their similarities rather than their differences. If Jack's angry reaction to the internment provoked institutionalization, shock treatments, and drug therapy, Togo did not internalize American values without paying a price, either. A recovering cancer patient, the eloquent former news editor had five surgeries and radiation treatment to remove a tumor from his throat. The film ends as Jack reads a letter of apology from President George Bush that is enclosed with a \$20,000 check he accepts as redress, a sum described on the sound track as "a small amount."

Steven Okazaki's Academy Award-winning *Days of Waiting*, though stylistically more conventional as a documentary, deals with an unusual subject, the experience of a Caucasian woman named Estelle Ishigo, who accompanied her Japanese-American husband, Arthur, to the concentration camp at Heart Mountain, Wyoming. The unwanted child of artistic parents in San Francisco, Ishigo represents the American standard of feminine beauty. A childhood photo shows a captivating young girl, dressed in white and wearing a locket, with masses of blonde curls, large eyes, and a bow-shaped mouth. She grows up alienated, however, and lives a tough life on the streets before attending art school and marrying Arthur Ishigo.

Okazaki uses photographs and actuality film to dramatize the relocation experience of the Ishigos, whose story is narrated in the first person by a voice-over that the viewer assumes to be Estelle. The narration is based on her illustrated book, *Lone Heart Mountain*. Although Okazaki occasionally dissolves or cuts from archival footage to Ishigo's drawings, he reserves her artistic legacy for the middle section of the film in order to show the desolation of 12,658 people crowded into hundreds of barracks on one square mile of wasteland. A succession of drawings and watercolors shows "long lines of people waiting to be fed . . . icy winds and bitter cold, shabby, shivering people with patches on their clothes . . . people with nothing to do but watch the sunset." Consisting mostly of shades of black, gray, and brown, Ishigo's palette becomes a study in brilliant contrasts to show sunsets that drench an otherwise barren landscape with color. The plaintive strings of *Cantus in Memory of Benjamin Britten* echo on the sound track. Determined to bear witness and to "write and draw what I saw," Ishigo remains at Heart Mountain until it is closed in November 1945. Allotted \$25 and transportation, she and her husband move from the internment camp to a segregated trailer camp in Los Angeles.

A man who once dreamed of becoming an actor, Arthur Ishigo dies a defeated man at age fifty-five in 1957. Estelle Ishigo continues to live in poverty but enjoys a bittersweet moment in 1972 when her work is included in a California Historical Society exhibit of concentration camp artists. She is unable to complete her story, however. A change in voice-over, now in third person, informs viewers that in 1983 a group of Heart Mountain internees found Ishigo living in a cheap basement apartment after both her legs were amputated due to gangrene. At the end, she has been moved to a Hollywood convalescent home.

During her years behind barbed wire, Ishigo, ironically, found acceptance for the first time in her life and thereafter identified herself as a Japanese (*nihonjin*) rather than a Caucasian (*hakujin*). Accordingly, Okazaki's film represents marginality based on racial barriers but transcended by a common human bond. As a whole, all the works reviewed here reveal the ways in which those who are powerless to control the events shown in newsreels experience personal tragedy yet remain indomitable. Both Tajiri and Okazaki end their works with extreme long shots of the desolate wasteland that Ishigo captured in her art and that remain as images in the minds of Japanese Americans who contemplate the meaning of their past.

Sumiko Higashi

State University of New York, Brockport

Hoffa. Produced by Edward Pressman, Danny DeVito, and Caldecot Chubb; directed by Danny DeVito. Screenplay by David Mamet. 1992; color; 140 minutes. Distributor: Twentieth Century Fox.

“**A** working class hero is something to be, a working class hero is something to be.” So sang John Lennon in 1970. Bruce Springsteen, a blue-collar barroom rock’n-roller and the son of a bus driver born in a decaying industrial town across the bridge from the Asbury Park, New Jersey, was America’s most recent working-class hero. In 1992, Danny DeVito, himself a working-class kid from nearby Neptune, New Jersey, put forth James R. Hoffa, the long-time leader of the International Brotherhood of Teamsters, who is perhaps buried in northern New Jersey, as this (de)industrialized and increasingly non-union nation’s latest proletarian icon.

Played by Jack Nicholson, the character of Jimmy Hoffa lords over the screen. He snarls, he spews ethnic epithets, he curses, he busts the heads of scabs, he proselytizes, he emasculates a self-righteous Bobby Kennedy, and he makes a pact with the devil in designer suits, or, in this case, the Mafia. He did all of this, we are told, not out of greed or because of an insatiable appetite for power but for the working *man*. Hoffa, as seen through the stylized lenses of DeVito’s direction and as heard through the tough-guy talk of David Mamet’s screenplay, is a princely Machiavellian warrior for white working-class males. (There are no people of color or women in this tale.) He “did what he had to do” to advance the righteous cause of trade unionism. Abiding by the law is the luxury of the privileged, many of whom, like the Kennedys, we are reminded, violated every statute on the books to gain their wealth. The powerless, meanwhile, had to scrape and claw to get their fair share, and in so doing, they were noble. This is the film’s morality. (Sean Wilentz makes this crucial point in his review of the film in the *New Republic*, February 1, 1993.)

Reviewers have been divided on the merits of *Hoffa*. A handful have praised the film, and a few more have applauded Nicholson’s robust, if nonetheless rather static, performance. Many, however, have criticized DeVito and Mamet for distorting the “facts.” Labor journalists, several of whom had covered Hoffa’s career and felt the sting of his ire, have taken it upon themselves to remind us—the uninformed and vulnerable masses—of the Teamster leader’s unheroic public record. (See A. H. Raskin, “Was Jimmy Hoffa a Hood? Or Was He Robin Hood?” *New York Times*, December 20, 1992; and Dick Meister, “The Film’s Hoffa Is a Hero, But He Sure Wasn’t in Real Life,” *Philadelphia Inquirer*, January 8, 1993, sect. A, 13, col. 1.) Indeed, they have assembled a list of Hoffa’s sins omitted from the film, including his dictatorial grip over the Teamsters, his usurpation of union democracy, his willingness to climb into bed with thugs and criminals as long as they remained loyal to his command, and, finally, his personal paranoia. Some have objected, moreover, to DeVito’s role in the film. The actor-director plays Bobby Ciaro, a composite character and the narrator of the tale, who, between unexplained liaisons with prostitutes, represents a model of Teamster fidelity. He will do

anything, no matter how brutal or vindictive, for our hero, and thus for labor. Still, Ciaro's "fictiveness" has intrigued commentators more than the character's ethics.

Such unself-conscious, even sanctimonious, rancor about facts might seem out of place to many historians. It should not require adherence to the canon of poststructuralism to appreciate that biography is never "the truth and nothing but the truth." Biography, whether it be written or visual, is a reconstruction of a life carved out of accumulated evidence and chiseled by the author's—or, in this case, the director, the screenwriter, and the actors'—beliefs, values, and political commitments. Biography, in other words, is the creative interaction between "facts" and analysis and author and subject. Perhaps, then, the question to be raised of *Hoffa* is not whether it is true, whatever that means, but whether it tells us something of importance about the past and present.

In *Hoffa*, as in other recent, Hollywood-based biographical epics about champions of the oppressed, the protagonist's private thoughts and personal relationships are not explored. Instead, the main character of these films functions as a representative figure. Mahatma Gandhi, in Richard Attenborough's 1982 film, for example, is the Indian nonviolent freedom movement; Malcolm X, in Spike Lee's 1992 account of the life of the slain activist, stands for all African Americans; and James R. Hoffa embodies the American working class. Strangely enough, despite each film's sympathies with the struggles of ordinary people, their larger subjects are virtually silent. To be sure, we see workers in *Hoffa*. We catch a glimpse of strikers reassuring their leader with cries of "We're with you, Jimmy!"; we watch workers hoist a beaming James R. on their shoulders after his election to the presidency of the Teamsters; and, in one of the film's final scenes, we encounter determined truckers standing along a rural road on a gray morning to greet Hoffa and Bobby Ciaro on their way to prison for jury-tampering. We see working people but do not hear them. Don't we need to know why laborers would cheer for Hoffa, carry him on their shoulders, and line a road to applaud him?

Teamsters pledged their allegiance to Jimmy Hoffa, the film suggests, because he was both one of them and the man they wanted to be. He got right in the face of the boss and screamed: "Fuck you!" Certainly, the Teamster chief articulated workers' hopes and grievances in a gritty language that they understood and valued, but he did much more than that; and the film does a disservice to its hero and the laborers who followed him by focusing so tightly on Hoffa's image and rhetoric. Jimmy Hoffa brought home the bacon; for many, this was what trade unionism was about in post-World War II–Taft-Hartley America. He was a formidable negotiator both in public and in back rooms, and he was a brilliant and savvy tactician. Under Hoffa's leadership, the Teamsters' membership rolls ballooned to 2 million strong. They ruled the nation's highways and interstates, they earned more money and worked fewer hours than most "blue-collar" laborers, they purchased homes in the suburbs and traveled to Florida on vacation, and they sent their children to college and bought color televisions. For these advances, Hoffa earned the fidelity of most Teamsters. "Jimmy Hoffa did more for the truck driver than anyone else who ever lived," remarked a New York Teamster in 1958. "So far as I'm concerned," added a Detroit union member the same year, "I'm for the union 100 per cent." "Every year that old raise is there, and it has been ever since Hoffa took over" (A. H. Raskin, "Why They Cheer for Hoffa," *New York Times Magazine* [November 9, 1958]: 78). This certainly was not the view of the labor movement envisioned by socialists, but the Teamsters snatched what they could from America's burgeoning postwar capitalist economy. And they did not apologize for what they got.

Except when Nicholson barks out to a black-tie audience, "We have led the American working class into the middle class, and we intend to stay there," the film only alludes to Hoffa's talents as a union boss. (Nor does DeVito address the hero's relationship with the Trotskyist leaders of the Minneapolis Teamsters. It was from these radicals, after all, that Hoffa learned many of the techniques and strategies that allowed him to put the bacon on his Teamsters' plates.) By portraying Hoffa as the embodiment of workers and then downplaying his role as a union leader, DeVito marginalizes workers. He turns trade unionists into facile individuals who value style over substance and respond to bombastic rhetoric and gestures of bravado rather than concrete gains and benefits. What DeVito does not appreciate is that working

people understood the world they lived in. For better or worse, by the late 1950s, workers as well as Jimmy Hoffa grasped that trade unionism had changed from an instrument of social reform to a lever for the redistribution of wealth for their members.

The eerie silence of workers in this film is echoed softly throughout American society. Less than two months before *Hoffa* was released, the Democrats captured the White House for the first time in twelve years. But working people can barely be heard as the nation debates how to rebuild its battered economy. It is a shame that DeVito missed the opportunity to remind America of its past—a time when the labor movement stood at the center of public discourse and working-class heroes chose to fight for the working class rather than sing stirring songs about the demise of their home towns.

Bryant Simon

California Institute of Technology

Bob Roberts. Produced by Forrest Murray; written and directed by Tim Robbins. 1992; color; 102 minutes. Video distributor: Live Home Video, Inc., 15400 Sherman Way, Van Nuys, Calif. 91406.

Hollywood loves American politics—as a target. From Frank Capra's *State of the Union* (1948) to Robert Redford's *The Candidate* (1972), films have portrayed politicians as hucksters and campaigns as con games. *Bob Roberts* marks Tim Robbins' contribution to the genre. While skewering many American political practices, *Bob Roberts* ultimately is defeated, as George Bush was, by the demand for substance.

Robbins wrote, directed, and starred in this satire, which traces the 1990 senatorial campaign of a conservative folk singer promising to "Retake America." This mock documentary follows Bob Roberts on the campaign trail, telling his life story through interviews, "archival" newspaper clippings, and voice-over narration. The "documentary" captures supposedly spontaneous moments that are actually scripted parts of the film. This textured effect highlights the gap between the fictional reality and the image Bob Roberts seeks to create. It also raises questions about the differences in perspective between the actual movie audience and the imaginary audience watching the documentary.

The film begins with a well-executed campaign entrance. A security guard whispers into his sleeve, "It's showtime." Roberts, his guitar slung over his shoulder, waves to the crowd, which claps and chants "Bob, Bob, Bob, Bob." Behind the candidate/folk singer is an enormous American flag; in front of him, a churning sea of smaller flags and red-white-and-blue posters. Roberts sings his anthem: "Some people will work, some simply will not, but they'll complain and complain and complain." This is the first of many visual clichés evoking the style of another entertainer turned politician, Ronald Reagan.

Bob Roberts' songs are the keys to his success—and the best part of the film. In blending the sounds of the 1960s with the rhetoric of Reaganism, Tim Robbins mimics the conservatives who rose to power by mastering popular culture. Albums with titles like "Times Are Changin' Back" and slick music videos use folk melodies, gospel, bluegrass, even rap to repudiate the Sixties. Roberts sings, "I'm a bleeding heart—let's give money away; to lazy people in the slums—I'm a bleeding heart."

The documentary captures many characteristic moments of modern campaigning, from a platitudinous debate to the never-ending photo ops. Singing in a gospel choir, hosting a beauty pageant, and appearing on a "Saturday Night Live"-like show, Bob repeatedly blurs the line between entertainment and politics and between religion and politics. His campaign is both a concert tour and an evangelical crusade.

Bob and his consultants manipulate the "liberal news media," receiving adoring but vapid

coverage from adoring but vapid news anchors named Carol Cruise and Rock Bork. Roberts postures appropriately, chiding an interviewer: "I came here to speak about the issues, but I'll indulge your personality search."

Roberts and his people create a black-and-white world, pitting "good Americans" against "Communists" and "card-carrying members" of the ACLU. They run negative ads and spread rumors smearing the incumbent, an aristocratic liberal played brilliantly by Gore Vidal. They also release an upbeat 15-second ad, with three flowers blooming, that fades out into a rising sun. The narrator proclaims, "For a new day dawning—Vote Bob."

All these elements make the first part of *Bob Roberts* subtler and more piercing than other political satires, including Eddie Murphy's *The Distinguished Gentleman* (1992), which sends a con artist to Congress, where "real money can be made." Unfortunately, Tim Robbins pads his film with obvious plot twists, making the movie longer and more commercially viable but cruder and less convincing. Robbins links Roberts with drug smuggling, Iran-*contra* gun running, and the fall of the S&Ls. Eventually, Roberts is wounded, in an assassination attempt that frames a radical reporter investigating these connections. As *The Distinguished Gentleman* meets *The Manchurian Candidate* (1962), the movie loses credibility. An elaborate conspiracy develops that only Oliver Stone could believe—too many policemen and doctors would have had to collude for this to work.

These Machiavellian twists blur Robbins' message. As the movie develops, the two most prominent voices of the Left become as unappealing as their opponents. Gore Vidal's Senator Brickley Paiste is pompous and paranoid, pontificating about a national security state. Bugs Raplin, the crusading reporter, is hysterical, shaking, neurotic, hunted. Only Robbins and his Hollywood crowd would view these zealots as heroes. Unwittingly, Robbins echoes E. J. Dionne's complaint that "Americans hate politics" because both liberalism and conservatism have gone stale.

By setting the movie during the build-up to the Gulf War, Robbins demonstrates that he genuinely fears a national security state. "This world turns its back on God, we must fight to protect him," Senator Roberts sings at a Washington dinner of the military-industrial establishment, celebrating his electoral victory. During this dinner, the camera zooms in on the folk singer, supposedly paralyzed from the attempted assassination, tapping his foot. The entire assassination episode is revealed as a lie. The movie ends on that night, January 15, 1991, with Roberts' fanatics cheering the murder of the framed reporter. The camera pans back from the mob, floating over Washington's monuments and zooming in again on the Jefferson Memorial. With Roberts singing in the background—interrupted by Persian Gulf updates—the camera focuses on Jefferson's vow, etched around his monument, to oppose "every form of tyranny over the mind of man."

Ultimately, then, *Bob Roberts* degenerates into caricature. Tim Robbins captures the race-baiting and selfish subtexts to Reagan's appeal: "Society's fault, I don't have a job, society's fault, I am a slob; Give me welfare, let me be," Roberts sings, "Hey, bud, you're living in the land of the free, no one's going to hand you op-por-tu-ni-ty." Still, Robbins' interpretation of the Reagan era is too simplistic and partisan.

In fact, the more the film lampoons Ronald Reagan's America rather than modern American campaigning, the more outdated it seems. Roberts' 15-second campaign commercial promising a new dawn in America echoes Reagan's 1984 commercials promising "Morning in America." By 1992, when *Bob Roberts* was released, such appeals were in disrepute. Paul Tsongas, Bill Clinton, and Ross Perot made 1992 a campaign of policy books, not just photo ops, of information bits, not just sound bites.

Bob Roberts, then, is an anachronistic historical source, a relic of the 1980s released in the 1990s. It is itself part of the backlash that demanded more substantive campaigning. The film chronicles many of the tactics central to the Reagan Revolution. It also highlights how much the historical memory of the 1960s overshadowed the 1980s, with Roberts emerging as the Reaganesque "rebel conservative." Still, these nuggets are obscured by the film's superficial and mean-spirited tone. Robbins reduces conservatives to lunatics, liberals to hysterics, and the American people to boobs. This slash-and-burn satire unintentionally complements a critique

of Hollywood that Robbins starred in as well. Robert Altman's *The Player* (1992), starring Robbins as a venal producer, showed how Hollywood operates; *Bob Roberts* shows what Hollywood thinks of its audience. Like the political stick-figures who populate so many movies, Hollywood seems to believe that Americans deserve what they get.

Hollywood's contempt for political campaigning fits into a broader tradition of American political criticism. Americans have long mourned the deterioration of political dignity while boasting about the democratic process. As with far too many of these critiques, *Bob Roberts* betrays an underlying disdain for the people and the process it intended to protect.

Gil Troy

McGill University

American Me. Produced by Sean Daniel, Robert M. Young, and Edward James Olmos; directed by Edward James Olmos. Screenplay by Floyd Mutrux and Desmond Nakano. 1992; color; 125 minutes. English with some Spanish. Distributor: Universal and Universal Home Video.

A*merican Me*—no relation to *American Me* by Beatrice Griffith (1947)—is an extraordinary attempt to reconstruct the lives of a group of Chicanos as they undergo a brutal transition from adolescents in the barrio to adults at Folsom State Prison, Sacramento. The intent of director, producer, and star Edward James Olmos was to create an anti-gang film. The point is dramatically and graphically repeated in vignette after vignette about the hopelessness and violence of gang life in or out of Folsom. To write that *American Me* brings to the screen the most intensely bellicose scenes of the inner world of the Mexican Mafia is to understate the raw power of this film.

American Me is an encounter with the world of the hardened criminal that has no equivalent in public life. The key moments are about brutal turning points that change imminent defeat and humiliation into triumph. The low moments are about missed opportunities, loyalties betrayed, and miscalculation turned to tragedy. The plot is clear: before *la eMe* (the Mexican Mafia), no one protected the homeboys in prison. With *la eMe* came respect, power, control, and a measure of freedom. *La eMe* controlled the inside (Folsom); and if it could control the outside (the streets), then it would command a say in the destiny of the barrio.

American Me is about a distorted effort to become part of the American Dream. The movie is not about the renunciation of the dominant society that is sustained in the culture of the barrio. It is not about the ordinary struggles for survival that typify the Chicano experience as presented in *Stand and Deliver* (1988). Notwithstanding the obligatory shots of women cooking, women crying, women pleading and dying of broken hearts, or of fathers frozen by failure and lost dreams, or of young *vatos* ready to join the madness of drive-by shooters, *American Me* is about gang culture rather than Chicano culture.

There is no ambiguity about the fact that *la eMe* has more in common with the Aryan Brotherhood and Black Guerrilla Family than it does with *La Raza*. Indeed, racial lines are blurred, as seen in the characters of "El Japo" (a Japanese American) and J. D. (a white man)—both members of *la eMe*. The Italians contract the Blacks to hit *la eMe*; *la eMe* contracts the Aryan Brotherhood to retaliate against the Black Guerrilla Family. The issues are about controlling goods and services—not about race. Even the portrayal of the Mexican Mafia and Neutra Familia as doomed entrants into their respective variants of the American Dream is questionable. No matter how much we agonize about the evils of gangs, no matter the righteousness of our moral imperatives, the historical fact is that gangs do work, they do accomplish their goals, and people do enjoy and derive a sense of pride and identity from gang

membership. Gangs are one of the consequences of our socioeconomic structure. *American Me* tells us that crime does not pay. But the reality is that the Mexican Mafia has succeeded in spite of what some may consider its irreversible pathology and in spite of our wish to make the nightmare go away.

What, then, is the value of *American Me* for historians? Here, the symbolic, the language, the action, and the nonverbal conventions carry the day. The glares, postures, gestures, the repeated shots of executions with make-shift shivs, the *chuco* strut, the patois of *caló*, the bloated attitudes about respect and pride, all leave a searing imprint in our hearts, throats, and guts. Equally palpable is the unnerving power of Santana as played by Olmos. Santana's autobiography in narrative form would be at best uneven. But as acted out by Olmos, it fills a gap in our ability to visualize a rendition of an immortalized captain of *la eMe*. We have Floyd Mutrux, Desmond Nakano, Sean Daniel, Robert M. Young, and Edward James Olmos to thank for what is, after all, a document about the popular culture of our own times.

Mauricio Mazón

University of Southern California

Imagining Indians. Produced and directed by Victor Masayesva, Jr. 1992; color; 30 minutes. English with Hopi, Cheyenne, Lakota, Navajo, and Blackfoot. Distributor: Victor Masayesva, Jr., P.O. Box 747, Hotevilla, Ariz. 86030 (602) 734-6600.

Talented filmmaker Victor Masayesva has put together a challenging combination of visuals, interviews, voice-overs, and editorial comment in this critical examination of the portrayal of Indians in American (read white) popular culture—especially film. At its best, *Imagining Indians* is provocative, passionate, and compelling. At its worst, the film is ponderous and crudely essentialist.

First the best, which I think outweighs the other. The film is wrapped around a weird, almost psychedelic, dream sequence, a sort of vision quest gone awry, wherein a young Native American woman undergoes some major surgery at the hands of a fawning white dentist. The dentist, who is crudely displayed as the white male oppressor working on and working over the Indian woman, also expresses the "What can I get from Native American culture and history?" narcissism of white liberalism. (He finds the "spiritual lifestyle you guys had" to be "very inspirational" and wants to cash in on a New Age/Indian spa and resort.) Good thing the woman turns on him at the film's close with one of his own dental tools. In any event, her tense dependency in the chair and a grimly administered anesthetic induce a dream sequence made up of the film's myriad images, interviews, and dialogue: all designed to make us think again about how Indians have been depicted in American film. The dentist-as-torturer metaphor may be a bit overdone, but the scenes in the office are good comic tragedy, complete with cowboy-and-Indian posters on the wall and the piped-in Muzak made up of nineteenth-century genocidal rantings of American politicians and military heroes.

The sequences themselves, those vignettes that form the woman's pained dreaminess, are of mixed effectiveness. Opening scenes about the indifferent treatment of Indian actors and extras in *Dances with Wolves* (1990) fall flat and could have been placed in the middle of the film (or done away with entirely). But the more subtle explorations of Hollywood's ability to create a new Native American dependency in the cultural heritage business are well done. Cultural appropriation through film and by film companies is a worry not only of Victor Masayesva but of the many talking heads he puts into the film. All are rightly concerned about the extent of white perversion of the Indian past and those things sacred to the Indian, even the whole "Indian-ness" that is being "imagined" in various popular culture genres. This refrain is

heightened in a funny sequence in which Masayesva himself wanders into the tourist art market of Santa Fe. Somewhat cruelly perhaps, he traps white tourists in the same way that Indians have been trapped by the dictates of the American pop cultural market: as single-dimensional, non-thinking, even ahistorical beings. Excited tourists fresh from the auction floor are asked what it is they are doing. They gush embarrassingly inadequate justifications and rationalizations of cultural commodification that go well beyond any "some of my best friends are Indians" objectification.

There are moments of relaxation in what is, at times, a relentless caricature of whites as moronic Indian-lovers and Indians as wronged victims of land theft, heritage theft, and religious theft. One thoughtful Indian actor admits that cultural commodification as expressed by the current film fascination with Indians has its short-range gratification in terms of jobs for Native American actors: "If a white man didn't tell the story, then the story wouldn't have gotten made," he reflects pragmatically. When Indian filmmakers have the wherewithal to attract wealthy producers, he says, then perhaps the Indian point(s) of view will receive greater representation. But, of course, what he treats as an eventuality may be only a naïve hope. Then again, Masayesva is in part saying that the box office definition of much of American filmmaking may not be appropriate for Native American films. Or, at the very least, that it is wrong to expect Indian filmmakers to make certain types of films.

This last point is brought to the fore in some interesting ways. Victor Masayesva is a good filmmaker and commands first-rate technical skills. Yet a number of the scenes in the film are deliberately askew when held up to the yardstick of "classic" documentaries. Some of the camera work in the interview scenes is sloppy, beyond even current in-vogue *vérité* stylings; some moments could have been edited with a firmer hand. Yet other scenes are dazzling, such as the endless film scroll of Indian movie posters moving behind two women talking about Robert Redford's dealings with the Hopi in planning for the film *The Dark Wind*. At times, Masayesva is saying that he will not consciously entertain us as Indian documentary filmmakers traditionally have: he offers little of the stock-in-trade, wide open, Indian country spaces, not much dancing or other ceremonies, few proud old people looking back on the days of traditional Indian integrity. In the end, some of the nontraditional filming is overdone, but when Masayesva does show off, as in the scrolling sequence, he makes up for the early excesses with a forceful, impressive kind of cinema.

Back to our dental patient. At the close of the movie, she moves toward the camera, the menacingly nerdy dentist oppressor slumped in the corner. With the same tool that was first bloodily used on her, then used against the dentist, she attacks the camera lens with deliberate swipes and slashes. It is a good scene: fighting the injustices and threats of cultural appropriation and the violation of Indian integrity may have its violent side (dentist killing), but it also allows one to seize control of the tools of such appropriation (even, one supposes, when the filmmaker himself is Native American). The white man, as one Hopi remarks in the film, "got a sweet mouth, he got a sweet talk." We may all know that, even with a schoolchild's knowledge of Indian history. But, as this extremely provocative, sometimes annoying, film helps us to understand, there is an important arena in which Indian voices are being heard, too, voices that help explore the nasty ironies of that white "sweetness."

William Deverell

University of California, San Diego

City of Hope. Produced by Sarah Green and Maggie Renzi; written, directed, and edited by John Sayles. 1991; color; 129 minutes. Distributor: SVS/Triumph Home Video.

Seeing John Sayles' *City of Hope* brought to mind an excursion my father and I made a few years ago along the New Jersey side of the Hudson River. He was eager to show me the radical changes under way. And, indeed, replacing the old and deteriorating docks, warehouses, and working-class housing were snazzy high-rise buildings, townhouse developments, and shopping centers—as if a Riviera on the Hudson were being created. Of course, my father well knew the drive would bring out the social(ist) commentator in me, and I obliged with remarks about capitalism, Reaganomics, and the rich getting richer . . . He listened, not unappreciatively, but when I finished, observed how “a lot of money was being made . . . by the banks, the developers *and*,” with the emphasis added no doubt to fend off my predictable class analysis, “by the unions, too.” I had no intention of disagreeing. I was much too caught up in what I was seeing to offer a good argument, let alone an alternative. I also recall a sense of impotence in the face of the transformative powers of capital, and I wondered to myself, who knows what is to be done?

Knowing that *City of Hope* was set along the same stretch of riverside that my father and I had driven and believing that its theme was the same corporate urban renewal that had so mystified me, I was hoping that Sayles had some answers or, at least, some critical insights—especially since he himself had lived in the area for some time. His previous films had never afforded easy or comforting conclusions, but a shared characteristic of such otherwise diverse movies as *Lianna* (1983), *Brother from Another Planet* (1984), *Matewan* (1987), and *Eight Men Out* (1988), was that, however tragic their tales, they had spoken of possibilities. In *Thinking in Pictures: The Making of the Movie “Matewan”* (1987), Sayles had written, “If storytelling has a positive function it’s to put us in touch with other people’s lives, to help us connect and draw strength or knowledge from people we’ll never meet, to help us see beyond our own experience.” Although it renewed my anxieties, *City of Hope* is a stunning film, challenging us to think hard about the long decade of greed.

My father’s tour of the new urban landscape and the vistas of Manhattan was from the comfort and security of our car. Sayles gets us out of our cars and into the streets of “Hudson City,” a place in decline and decay but one already targeted by real estate developers for a revolution from above. *City of Hope* could easily have been conceived as yet another film about corporate ambitions and schemes directing our attention across the river to the boardrooms and bedrooms of Wall Streeters—indeed, the impetus for much of what transpires is a corporate syndicate’s desire to secure a piece of land occupied by a run-down project where poor black and Hispanic families live. But *City of Hope* is not immediately about corporate power and greed; in fact, except for a brief early scene in which the local assistant district attorney assures a corporate delegation that the projects will “come down” on time—having been promised a “campaign contribution”—there are no Wall Street lawyers in sight. Rather, the film is about the power and accommodations to power of the “lower orders,” the local elite and middle and working classes of the city, whose lives—however divided by class and, most especially, by race and ethnicity—are tightly bound up in a system whose most pervasive characteristics are corruption and dishonesty, a political order wherein the “common good” is arrived at when “the deal” is fixed.

City of Hope is actually a collection of deftly woven multi-character stories and entanglements far too numerous and complex to recount here. More significant is Sayles’ vision of contemporary urban America, which is dark and depressing and made all the more so by the fact that most of the “action”—a bungled robbery, police harassment of young blacks, a racially motivated mugging, an attack on a black community center, a torch-job, a protest march on the white politicians’ campaign dinner, and a climactic shooting—transpires at night. Watching *City of Hope*, I was reminded of Dante’s *Inferno*, especially those circles of Hell in which the damned

mete out their eternal punishments *to each other*. Sayles himself seems to allude to this at the outset when he has us descending from the upper floors of a building under construction to the streets of Hudson City below. I can easily imagine a sign being posted at ground level: "Abandon All Hope Ye Who Enter Here."

Sayles captures as well the urban policies—nay, the entire culture and political economy—of the Reagan and Bush administrations (the consequences of which we now can recognize in South Central and Crown Heights). Almost everyone in Hudson City is out to get something—more, ahead, or out—from you, for you, or on you; ambitions and aspirations made all the more urgent by the sense that you had better move fast or be swept under or aside when the developers take over. An arson fire takes the lives of a young mother and baby because no one knew they were squatting in the abandoned section of a building that was torched. As Baci, the mayor, explains to Joe Rinaldi, the owner of the apartments that had to "come down": "Accidents happen . . . Those are the kinds of people accidents happen to . . . Next couple of years, this town is going to be one big yard sale, and anyone with half a brain will make tracks and let the blacks and Hispanics duke it out for whatever is left."

No one stands fully outside the system. Even those who seek improvements and see themselves as battling corruption discover that they are already implicated in it and must accept it, work the fix, and hope to accomplish something before it catches up with them. Wynn, the black city councilor pursuing reform, learns that, to effectively mobilize the anger of the black and Hispanic "communities" against the Baci political machine, he must compromise his own values and ideals. He is persuaded to do so by Errol, the former black mayor forced to retire under threat of criminal prosecution. Errol counsels Wynn—while lining up a shot on an otherwise "white" country club golf course—if you want to "help your people," the best you can do is "lie and hope for twelve good years."

Presenting us with a polity characterized by graft and demagoguery, a civil society reduced to tribalism, an economy in which the organization of the working class is bought off or racist, and a popular culture in which the rebelliousness of youth is vented in drugs, crime, and racially motivated assaults, does Sayles afford us any hope that things could be made different? My father would have said that at least the developers were creating something new and surely preferable to the Hudson City revealed by Sayles—and, to be fair about it, there is no nostalgia, no lament for the passing of the old in *City of Hope*. Sayles does grant us some moments in which we can recognize the possibility that there might yet be some way out of the inferno, not only individually but collectively. But such moments are few.

Consider what Sayles leaves us with: It is night again. Anxiously searching for his son, Joe Rinaldi finds him on one of the unenclosed upper floors of the original construction site. Joe had agreed to the arson in order to get his son off on a robbery charge. They talk, acknowledging their shared guilt for the fire. Joe then sees that his son is seriously wounded. Promising not to leave him, he runs to the edge of the building and begins shouting for help. The only response comes from a deranged character in the street below shouting back up to him, mimicking his cry, "Help! Help! We need help!"

Romantically, I would have preferred a call for struggle, not aid. Nevertheless, *City of Hope* poses the essential historical question, whither America?

Harvey J. Kaye

University of Wisconsin, Green Bay

Featured Reviews

GERDA LERNER. *The Creation of Feminist Consciousness: From the Middle Ages to Eighteen-Seventy*. (Women and History, number 2.) New York: Oxford University Press. 1993. Pp. xii, 395. \$27.50.

This book brings to a conclusion Gerda Lerner's two-volume overview of "Women and History." The first volume, *The Creation of Patriarchy* (1986), located the origins of patriarchy in the formation of archaic states in the second millennium B.C.E. In Lerner's view, the historic creation of patriarchy twisted not just gender relations specifically but also human relations in general. As she tells the story, men's experience of gender dominance antedated and facilitated all other forms of social inequality; before there were slaves or subordinated racial groups or disadvantaged classes, there were women subjected to the sexual dominance of men. Moreover, although this male dominance was originally rooted in sexual and economic powers, its greatest buttress was ideological; in the dethroning of goddesses and the severing of the connection between women and the Divine, Lerner identified "one of the founding metaphors of Western civilization" (*Patriarchy*, p. 10).

Convinced of the power of idea systems in maintaining patriarchy, Lerner has turned in this second volume to the study of how women have bypassed or redefined or undermined "male thought" (p. 12). She elides more than a millennium between volumes one and two, starting this new study in the seventh century C.E. (after women's writings begin to survive in relative abundance) and ending in the late nineteenth century (with the appearance of organized women's movements). In the writings of diverse women across these centuries, Lerner has found either nascent or full-blown feminist consciousness, a term that she precisely defines as entailing an awareness by women that they belong to a subordinated and wronged group, that their oppressed status is not natural, that they must join with other women to effect change, and that they must create an alternative vision of egalitarian gender relations. Since the word "feminist" was not even commonly used until after the terminal date for Lerner's book, the subjects of her study rarely identified themselves as feminists, and

many, as she recognizes, would have rejected the label. Nevertheless, all these women—for example, Hrosvitha of Gandersheim, Margery Kempe, Glückel of Hameln, Emily Dickinson—"directly contributed to the development of feminist consciousness, whether they so intended or not" (p. 17).

Lerner's thesis is straightforward. Patriarchal ideology disempowered women not only by defining women as deviant but also by excluding women from ideological production. Lacking clear authority to speak, adequate education, and a known tradition of feminist critique, women accepted the ideological constructs of gender, colluded in gender oppression, and helped to re-create it generation after generation. This dreary situation changed only when female thinkers, developing feminist consciousness, began to attack the ideological power of patriarchy. Lerner seems to argue that this feminist attack developed in two stages. First, women asserted their right to ideological production. Some women found authority for their ideas in divine inspiration and mysticism, others spoke from the authority of motherhood, and still others wrote on the basis of creative genius alone. Yet although justifications such as these allowed women to write, they did not ensure the survival or dissemination of their writings. As a result, women's ideas were often lost, so that generation after generation "women's creations sank soundlessly into the sea" (p. 220) and each subsequent woman "had to argue as though no woman before her had ever thought or written" (p. 166). Second, from about the seventeenth century, women slowly created clusters of supportive authors and readers, slowly gained some access to higher education, slowly created spaces where they could think and write unencumbered, and slowly also acquired a fuller knowledge of women's history. With these developments, which are still coming to fruition, women "can fully think their way out of patriarchy" (p. 246).

This is a compelling argument constructed by a

distinguished and well-respected historian of women. As a synthesis, this book is a valuable tool for teachers, summarizing concisely the lives and writings of many female intellectuals and providing extensive and well-organized bibliographies. Yet as a synthesis, this book must also be appreciated for its breadth and vision, not mined for its specific detail. Errors are perhaps inevitable in a book of this scope. Readers will wince, for example, at finding the apostle Paul located in the second century C.E. (p. 124) or at reading that European population did not grow through most of the Middle Ages (pp. 121–22). Failures to keep up-to-date with historical literature are more regrettable than factual slips, albeit even more inevitable in this type of study. Lerner repeats such hoary myths about medieval women as their supposed subjection on their nuptial nights (if serfs) to sexual intercourse with their lords (p. 120) and their supposedly disproportionately high representation in heretical sects (p. 74–75). Inconsistencies of argument and logic are also common in works of synthesis, and Lerner's is no exception. Readers will wonder, for example, why Lerner, who argues that women's writings in the medieval and modern eras were often produced in isolation and suppressed or ignored, justifies beginning her study in the seventh century C.E. by noting that ancient writings by women were "isolated voices that found neither echo nor response" (p. 13). Readers will also wonder what Lerner really thinks of the single women (never married, divorced, or widowed) who have been so important in the creation of feminist consciousness. Lerner treats single women both positively and negatively, as a critical "precondition" for a development of feminist thought (p. 276) and as a sad example of how women have had to "pay such a [heavy] price for intellectual growth" (p. 11). These two views can be reconciled, but Lerner never tackles the task. These sorts of errors, misstatements, and inconsistencies are troubling, but they are probably unavoidable in a broad survey. Lerner's book neither abounds with them nor should be judged on that basis alone. Taken as a challenging synthesis, this study posits a broad history of feminist thought that will shape the field for decades.

Any work of synthesis is predicated on hard decisions about approaches, definitions, methods, and materials to be included and excluded. Lerner's decisions on these matters will be intensely discussed. Are all of these women really properly identified as feminists? Did long-unknown and long-ignored works really contribute in any substantial way to the development of feminist thought? How would a perspective less Western European open up the history of feminist consciousness? Is it fair and useful to label as "distorted and unhappy" (p. 179) the lives of women who eschewed heterosexual marriage and/or motherhood? Among the many provocative and fruitful issues this book raises, three especially merit consideration: the conflation of feminism and femaleness; the teleological basis of Lerner's argument; and

the overwhelming importance placed by Lerner on Women's History (which she capitalizes to denote the distinction between history as past events and History as the interpretation of those events).

Lerner has no hesitation in asserting that women and men, because of their different life experiences, had and have different ideas and different cultures. As a result, she often writes as if all women and no men were feminists. For Lerner, a true female experience seems to be a feminist one; throughout the book she speaks without distinction of "women's consciousness" and "feminist consciousness" (see both used on p. 14). For Lerner, a true feminist seems always to be a woman; consistently ignoring or downplaying the feminist efforts of men, she mentions, for example, John Stuart Mill only once and in passing. And for Lerner, the best female feminist seems to be one freed from reliance on even the best-intentioned men; she even suggests that supportive husbands, fathers, and brothers actually undermined feminist consciousness. I am entirely sympathetic to Lerner's assumption that most feminists have in fact been women, but how would her story look different if men were given a more comfortable place?

Lerner's argument progresses from the first hints of feminist consciousness in the written work of female mystics and women religious of the Middle Ages through the more assertive arguments for women's education advanced in the seventeenth and eighteenth centuries to the full-blown intellectual presence of feminists in the nineteenth century. This teleological vision shapes profoundly the structure of the argument. It forces Lerner to ignore women's complaints from the ancient world (many of which are preserved in writings by men), to elevate to feminist status some very questionable medieval writers, and to portray Emily Dickinson as the "perfection and culmination of centuries of women's struggles for self-definition" (p. 181) whose life was "in the best tradition of women's resistance to patriarchy" (p. 190). It also forces Lerner to see progress in feminist thought where there might be only changed intellectual context (as, for example, in the development of arguments about natural rights for women in the nineteenth century). Again, I am entirely sympathetic to the argument that there has been more feminist theorizing in the last hundred years than previously, but how would Lerner's history look different if it did not have to progress from emptiness to fullness but could instead move along a less certain and less Whiggish path?

In Lerner's view, the production of Women's History is a *sine qua non* in the development of feminist consciousness. When women in the past had no knowledge of Women's History, they faced "perhaps the most serious obstacle of all to their intellectual growth" (p. 12). Any feminist historian of women (like myself) would be sympathetic to this argument, for we certainly hope that our efforts will inform and effect progressive change in the present and future.

But do we overestimate our own work when we make History so important to the feminist struggle? Lerner is right to note that the "cruel repetitiousness by which individual women have struggled to a higher level of consciousness" (p. 281) is rooted in part in the obstruction and obfuscation of Women's History. But that cruel repetitiousness is also rooted in matters that this book (with its emphasis on idea systems) scarcely considers: economic inequality; household structures and marital practices; legal restrictions; male violence; reproductive technologies and controls. Is Women's History all important, or just important?

In raising these (and other) questions for debate by

historians of feminist thought, Lerner has made yet another invaluable contribution to the development of women's history. She has moved with courage and verve across many centuries and regions, and she has constructed a synthesis stamped with her own vision of women's past and women's future. Her book will be much debated and much revised, but most importantly, it has provided a source from which much future work will surely spring.

JUDITH M. BENNETT
University of North Carolina,
Chapel Hill

ERICH S. GRUEN. *Culture and National Identity in Republican Rome*. (Cornell Studies in Classical Philology, number 52.) Ithaca: Cornell University Press. 1992. Pp. xiii, 347. \$37.50.

When Rome conquered Greece in the second century B.C.E., did the Greeks turn the tables on their conquerors and achieve cultural hegemony? What happened when the Roman ruling class confronted the glories of Greek civilization? Did the Romans allow their culture to be subordinated to Hellenic culture? Or did the Romans use their military superiority to reject Hellenism?

These are the perplexing and ambiguous issues that Erich S. Gruen tackles in his latest book on the meeting of the Roman and Hellenic cultures. Gruen is well known for his work on Roman history and has published widely on the encounter of Rome with the Greek East in the third to second centuries B.C.E. His most recent book expands on a more limited treatment of the same topic in *Studies in Greek Culture and Roman Policy* (1990). Gruen asks how the Romans came to terms with a society that sparked fascination and admiration on the one hand and hostility and alienation on the other. For example, many Roman intellectuals and aristocrats (not always the same) welcomed Greek philosophers into their homes and employed them to teach their children, and many Greek religious cults were happily embraced, yet the Roman senate expelled Greek philosophers, orators, and professors on several occasions and banned the worship of Dionysus in 186 B.C.E. How do we make sense of these ostensibly conflicting actions?

In an effort to understand Rome's "groping toward cultural maturity and self-definition" (p. 2), Gruen undertakes a fresh examination of events and personalities, seeking to show how Rome strove both to participate in the broader cultural world of the Mediterranean and to exhibit its own primacy in that world (p. 223). Gruen's main thesis, driven home repeatedly in each of his seven chapters, is this: Rome expropriated Hellenic culture to serve its national

ends, to define the distinctive features of Roman values, and to enhance its civic and religious life.

Gruen argues his thesis by examining several areas in which the Romans developed a distinct identity while working through their confrontation with Hellenic myth, art, and literature. First, he investigates the foundation myth of Rome, a myth that combines Hellenic and Trojan ancestors in a wildly confusing amalgam of tales. The choice of a Trojan forefather in the person of Aeneas amid a welter of Greek founders reveals how the Romans carved out their own cultural legitimacy while still maintaining a strong connection to the Hellenic world. Gruen next focuses on a central figure in this inquiry: Cato the Censor, famous crusty curmudgeon and foe of Hellenism (according to most previous accounts). Cato is a conundrum, however: while attacking Greek philosophy, medicine, oratory, and poetry, he made it his business to learn as much as possible about Hellenic culture. Gruen seeks to explain this paradox, urging us to see Cato less as an implacable foe of Hellenism than as a clever statesman who used his vast knowledge of Hellenic culture to affirm Roman values and to articulate the distinct character of his countrymen.

Gruen then examines Roman art, in particular historical reliefs and veristic portraiture, and its relationship to Greek art. He corrects the commonly received picture of the booty-snatching Romans, interested only in financial gain and careless of another culture's great treasures. Instead, he sees Roman commanders who encouraged Greek artists in Italy, stimulated a taste for Greek art among their people, put the art they liberated to appropriate uses, and used Greek art to serve Roman civic purposes.

In chapter five Gruen explores the role that Roman drama played during the third and second centuries B.C.E. in expressing the values of the Roman aristocracy while modeling itself closely on Greek drama.

The second-century comic writer Terence, who moved away from the slapstick of Plautus to a more refined and sophisticated comedy, is central to this discussion. Gruen sees drama as a cultural artifact and, like art, a medium through which the Roman ruling class asserted its dominance over Greece, made strong political and cultural statements, and showcased its own cultural values.

Gruen's final chapter, his "coda," focuses on the reaction to the highbrow takeover of Hellenic culture by the Roman elite. In the newly developed genre of satire, Lucilius takes the Roman nobility to task for their pretensions and arrogance, by-products of their attempts to prove their superiority to the Greek intellectual tradition they all (or almost all) admired and expropriated. Lucilius is of particular value as a window to this changing world because he is an outsider (always the satirist's stance) who exposes the consequences of Rome's encounter with the Hellenic East.

Gruen's longstanding interest, first in Rome and then in its intersection with the Greeks in the last few centuries of the republic, has produced eight important volumes. The first in his current "series," *The Hellenistic World and the Coming of Rome* (1984), is a two-volume work that takes up many of the issues discussed in his two more recent volumes: philhellenism, patronage, Roman expansion and imperialism, and the Roman confrontation with the Hellenic East. For those working on the history, literature, or art of this period, Gruen's books are valuable. They are a model of their kind: meticulously documented, full of informative notes and arguments, and careful in their exposition of details.

Yet in certain ways Gruen's book is representative of an old-fashioned scholarship that shows little interest in the larger questions of history or in the kinds of sophisticated formulations that contemporary critics and theorists see as indispensable to any explanation of a nation's culture. As a feminist scholar who has become acutely aware of the necessity of allowing for multiple interpretations of events and personalities, particularly events as vexed as Gruen describes, I would question Gruen's often professed aim of seeking a resolution that probes past paradoxes (see, for example, pp. 2, 52, 62), even sometimes using formulations like "The truth lies elsewhere" (p. 268) and "Warmington . . . has it right" (p. 300, n. 150), which seem to indicate a belief that it is possible to discover an absolute and unarguable historical truth about complex events.

Gruen makes no statement about his methodology. In general, his plan is to lay out either two different explanations of a problematic issue (sometimes exaggerating the two sides) or a previous but unsatisfactory explanation for an event and then to reach a new and better solution (see pp. 183, 188, 202, 227). Gruen is sometimes so Romanocentric that he seeks to explain away actions and statements that seem clear manifestations of antihellenic behavior, as when

he calls Cato's alleged antihellenism "a mere matter of style" (p. 262). Until the last chapter, Gruen tries so hard to protect the Romans from charges of being boorish and uncultured and to do away with the antihellene/philhellene dichotomy that he sometimes oversimplifies complex situations (a charge that he levels at others) and makes insupportable statements. For example, Gruen says that "Romans claimed participation in a shared culture of which they were both the heirs and the custodians" (p. 240). One wonders what the Greek view of this would have been. The very language that he uses of the Roman takeover of Greek culture—"control," "commandeer," "exploit," "confiscate"—calls into question the shared nature of this enterprise.

In his attempt to make overly tidy a very messy situation (one reason why so many historians have avoided dealing with the Hellenistic period until recently), Gruen does not sufficiently problematize his sources or statements, but instead seeks airtight solutions to questions we simply do not have enough information to answer. With so much fragmentary evidence and conflicting information, one must be particularly cautious about basing conclusions on speculations. As Gruen himself notes dozens of times, the attitudes of Rome toward Hellenic culture were complex, not simple.

Gruen's seriousness of purpose sometimes causes him to devalue some wonderfully fanciful or grotesque stories that beg to be enjoyed and discussed more fully (pp. 40, 230). For example, one version of the foundation myth has Romulus and Remus born from the union of a servant girl and a disembodied phallus. Gruen simply refers to this as a "peculiar narrative" which demonstrates the proposition that, even in the late Republic, a bewildering diversity of accounts existed (p. 40).

There are other problems that one wishes Gruen had taken into account. First, I miss any reference to the larger issues of history to which Momigliano referred in his Sather Lectures from 1961–62 (*The Classical Foundations of Modern Historiography* [1990]), in which he talked about the need to discuss "the nature, function, limits and methods of historical research" (p. 153). Second, history often seems to shade into literature, myth, and legend. Where are the boundaries? When and how can satire be used as a historical document? Third, Gruen often firmly separates politics from culture without ever defining his terms. Fourth, I wonder if history can any longer describe only the world of the Roman male aristocracy without at least giving some attention to other classes, genders, and cultures in Rome and the Mediterranean that have been omitted from consideration.

I would also quibble over some minor points that can only be briefly mentioned. For example, in Gruen's discussion of Greek art (chapter 4), he claims that the Romans respected Greek artists as well as Greek art (p. 140). I would argue that the Romans differen-

tiated sharply, giving respect to the art but not the artists who produced it. Also, Gruen plays down too much the role of patronage in the workings of Roman society of the third to second centuries B.C.E., especially in his discussion of the role of Roman officials in producing drama.

Having said all this, I would like to return to my original words of praise for Gruen, who has taken on

so many difficult and vexing issues in his volumes and given all classical philologists and historians much to debate. Hence my disagreements above. One does not bother to disagree with a negligible or unimportant book.

BARBARA K. GOLD
Hamilton College

ALAN S. MILWARD. *The European Rescue of the Nation-State*. Assisted by GEORGE BRENNAN and FEDERICO ROMERO. Berkeley and Los Angeles: University of California Press. 1992. Pp. xv, 477. \$45.00

With the publication of this immensely rich book, contemporary Western European integration studies are now dominated by the work of Alan S. Milward. Although his prior study, *The Reconstruction of Western Europe 1945–51* (1984), presented one of the first systematic views of postwar rebuilding, it was less than totally convincing and less than thorough on the so-called formative era of the unity movement. Several contributions since, notably the monographs of Michael J. Hogan on the Marshall Plan's impact on Europe (*The Marshall Plan* [1987]) and John Gillingham on the formation of the European Coal and Steel Community, or ECSC, (*Coal, Steel, and the Rebirth of Europe, 1945–55* [1991]), plus the valuable collections by Klaus Schwabe, Raymond Poidevin, and Enrico Serra, have been instructive and increased overall knowledge about the rise of transnational sentiments and institutions. Milward's searching account of the origins of the Treaty of Paris that gave birth to the ECSC and the treaties of Rome that did the same for the European Community (EC) is a full revisionist interpretation that cuts through the rhetoric of many past, often trendy, and mostly orthodox volumes on the genesis of integration. It may contain a defect, but it is, nevertheless, in sum, a tour de force.

In what will most likely become a classic on mid-century European developments, Milward has written the first historical investigation on the causation of integration. By raising old questions and posing new answers, he will certainly stimulate a titanic historiographical debate. Arguing a fresh and highly original thesis, he asserts that the traditional nation-states were being chiefly defended and recast to adapt to new circumstances by those who originated regional integration. Their objectives were not to construct new cross-frontier organizations, but to retain the now threatened nation-state system. Milward's contribution is explaining the emergence of a new national power paradigm that included compulsory intergovernmental institutions that would assist the postwar nation-state in achieving the more difficult and complex answers to the problems of the contemporary national political economies of the region.

Contrary to what has been commonly assumed in most standard works, Milward insists that economics followed politics in that domestic political (and social) requisites prescribed strategies for the economic domain. He is steadfast in denying any primacy to external affairs—that is, the Cold War, the Soviets, and the Americans—in this decision to employ pan-European structures that would prop up and reinvigorate the state. Milward counters what he calls a fiction—that a European revolution occurred in which national politicians undermined or diminished state power with new trans-state organizations—with his novel historical outlook. His argument is that after 1945 “the European nation-state rescued itself from collapse, created a new political consensus as the basis of its legitimacy and through changes in its responses to its citizens which meant a sweeping extension of its functions and ambitions, reasserted itself as a fundamental unit of political organization” (p. 3). That this state-salvaging demanded some limited surrenders of national sovereignty to a supranation should not mislead the historian, he states, to believe that regionalism was replacing the state with another core form of governance.

The creation of “Community” across state frontiers, Milward demonstrates, was a politically necessary, consciously plotted arrangement that left most political powers within the nation. Thus, Milward believes that the internal state environments in the Federal Republic of Germany, France, Italy, and the Benelux countries ultimately determined state decisions, not beliefs or convictions about the empowerment of federal communities. He calls the preponderant integration literature “myth making” when it states that European intentions were to form a body politic with supranational authority that superseded those of its members. In effect, those politicians agreed that they were collectively responsible for solving problems anew in order first to save the nation-state. He demonstrates that this meant erecting some cross-border institutions that pooled small portions of state power, which would not just maintain the nation but also reinforce it.

This book is an enviable achievement because of the extent and depth of archival research in national and regional centers and the multilingual talents therefore necessary. It is research work on an unprecedented scale and in six languages. In many respects, much prior postwar history in this field was provisional due to closed archives. When one considers the enormity of the released national documentation since the mid-1980s, Milward has produced an impressive history characterized by relentless research into new primary sources. His pioneering study does not just use these new materials, but it makes a forceful and convincing historical synthesis that is grounded in realism and contains intelligent, perceptive, and coherent judgments. Even if subsequent evidence casts doubt on his conclusions, he has significantly enlarged the student's angle of vision with a work of the highest scholarly rank.

Milward was for a time at the European University Institute at Florence, where he gained great advantages from both the explorations of other integration researchers and the materials unearthed when the thirty-year law covering the depositories expired on the 1950s documents. The close collaboration and interchange with the specialized inspections of other scholars is reflected in two of Milward's major points. It does not diminish Milward's success in any sense to cite his drawing on the knowledge of others, for instance Richard Griffiths and Michel Dumoulin on the Benelux states, in forging a central part of his thesis. In the Belgian coal sector study and Dutch farm and customs-union concerns, Milward gives us a deeply detailed understanding of the paramount roles of these small states. In two weighty case studies, he first explains Brussels' policy on the Walloon Borinage and Flemish Kempen coal regions as national readjustment through shifting the burden of protection to a supranational entity. Similarly, Milward then draws a portrait of Dutch influence and close interaction with Bonn in bringing about specific national bargains and trade-offs that would increase the national welfare and interest at the lowest cost possible in terms of concessions to integration vehicles. The Beneluxer place in the reconstruction period is best indicated by the author's choice of the real instigators in forming the "dual allegiance" concept or integration/nation-state mix. They are not the highest level leaders but rather two Dutch functionaries, Dirk Stikker and Johan Beyen, and three Belgian politicians, Achille van Acker, Albert Coppé, and Paul van Zeeland, with Milward's selection of Beyen as the closest to the true architect of the *mélange* idea.

Several other chapters make contributions of the first order. Although Milward only briefly alludes to interwar economic influences on postwar politics, he deftly links them to his thesis. Insufficient attention to the activities of wartime governments-in-exile strangely denies him the opportunity to fortify his Benelux track. Milward does, however, deserve great credit for his laudable dissection of integration theory

wherein Karl Deutsch, Ernst Haas, and Leon Lindberg, among others, are shown to have simplified history unacceptably. That chapter deserves the attention of all integration scholars, especially political scientists. So, too, does he gain top grades for his ambitious and thought-provoking explanations on the role of the export trade, the European farm dilemma, and, most importantly, the nexus of it all, Germany's needs and wants. For those who find the Common Agricultural Policy difficult to comprehend, Milward's expert treatment of how it was devised to protect inefficient, backward, but nevertheless politically important farm regions is first-rate. He is not the first to point out this trio of determinants, but he has woven the segments quite efficiently and soundly placed them in the context of a broad analytical framework. There is also much to savor in the concluding chapter on the split between the British and the continentals. The depiction is one of British inability to perceive how "Little Europe" was devising a new governance system that still had at its heart the nation-state and was not engaged at all in a comprehensive move to merge national sovereignty into supranational institutions. London misread integration, says Milward, and was weakened substantially by not joining the two communities. His most telling evidence from new Bank of England archives and the Public Record Office (materials on the Cabinet, Prime Minister, Board of Trade, Foreign Office, and Treasury) is devastating on British prejudice and blindness toward the incipient European economic order. Particularly revealing is Harold Macmillan's statement in 1959 about the three devils attempting to mislead the United Kingdom, "the Jews, the Planners, and the old cosmopolitan element" (p. 432).

The book does suffer from two handicaps, one a major deficiency. First, much of it is too wordy, and sometimes it is dense. Long, convoluted sentences mar the book. Milward writes with boldness, but he can be diffuse. The central and more salient fault, however, is the unpersuasiveness of the chapter on elite thinking and motivation. He attempts to convince the reader that Jean Monnet, Paul-Henri Spaak, Robert Schuman, Konrad Adenauer, Alcide de Gaspari, and their colleagues were merely national statesmen adhering to national wishes and aims and doing so by employing national instruments of power. We are told that they had little if any genuine convictions of a federalist or functionalist nature. Milward does not overcome the evidence that these men would place substantial restrictions on national sovereignty for *both* the national good and the now interdependent and collective regional economic welfare. It appears that his suspicions about most oral history sources play a critical part in his rejection of any integrationist leanings in these "European saints." Milward is at his best with Schuman, but the pages on Monnet, Spaak, and de Gaspari are the most disappointing, omitting evidence that they were not mere pragmatists or engaging in integration rhetoric

for political purposes. Building genuine trans-state political groupings for economic areas, we now know, was becoming acceptable to these leaders. Milward may be contributing significantly with his point that this all had to be achieved in the 1950s in a fashion that accommodated state power, but his notion that the national leaders were only moving minor, concessionary, and inconsequential powers to new integration institutions defies the evidence, for example, of the High Authority of the ECSC.

The rest of this work is a single organic entity, but not the section on the "Great Men." Milward may rarely speculate and never stray from the record elsewhere in the book, but he does much of both in this chapter. If in this area the evidence is contradictory, Milward might best have left unanswered the unanswerable questions. There is therefore a weak segment to this book, and although it does detract

from Milward's achievement of an absolutely firm set of conclusions, the enormity of what he has accomplished cannot be contested.

Milward has produced a marvelously intelligent reappraisal that examines and analyzes Europe's reaffirmation of the nation-state as the cardinal organizing unit of midcentury society. It will be vigorously debated for years because the author demonstrates persuasively that the "Community" ideas and institutions were meant then to reinforce the state, not to supersede it. It may not appeal widely because of its revisionist bent, but the study is mandatory reading for all interested scholars, whatever their discipline. On the whole, no other book has given us as definitive an account or as compelling an interpretation as does Milward's prodigious feat of historical scholarship.

PIERRE-HENRI LAURENT
Tufts University

JUDITH R. WALKOWITZ. *City of Dreadful Delight: Narratives of Sexual Danger in Late-Victorian London*. Foreword by CATHARINE R. STIMPSON. (Women in Culture and Society.) Chicago: University of Chicago Press. 1992. Pp. xiv, 353. Cloth \$35.00, paper \$15.95.

The cover of Judith R. Walkowitz's important study reveals a well-dressed woman standing in a courtyard staring boldly at the viewer. In dress and bearing she resembles one of the many New Women who flocked into the East End to help the poor or lift up "fallen" women. As Walkowitz's many-layered book makes clear, appearances deceive. For this respectable-looking figure was a prostitute, Mary Jane Kelly, whose notoriety rests on her role as Jack the Ripper's last known victim. The story of her decline from an Irish-born miner's wife in Cardiff to a "common whore" working the streets and pubs of the East End (after a short stint as an up-market "sex worker" in the West End) is known to all "Ripperologists." On the night of November 9, 1888, she met London's most feared citizen and suffered a frightful death. The contrast between this lithograph and the police photographs of Kelly's eviscerated body—a corporeal object barely recognizable as female—simply cannot be expressed in words.

The liminality of this illustration symbolizes Walkowitz's approach to London's culture and society in the 1880s—a decade she regards as pivotal in shaping "modern" attitudes toward sexual politics. She presents a kaleidoscopic view of London in which human relations, texts, and contexts change color and form as she holds them up to the light of close readings. Pursuing the issues of gender conflict and sexual danger within a feminist framework, she explores middle-class discourses about sexuality, identity, power, urban space, prostitution, marriage, philanthropy, and spiritualism. She draws on the work of

feminist and new cultural historians to map the landscape of gender and sexual relations in this era. But it is not "Jack," the "fiendish" Whitechapel killer, who occupies center stage. Instead, the leading male actor is William T. Stead, the dynamic editor of the *Pall Mall Gazette* from 1883 to 1890, whose sexual obsessions run like a *basso continuo* through this polyphonic work.

Combining feminist theory with Foucauldian ideas about discursive practice, Walkowitz sees Stead's famous "sexposé" of child prostitution, *The Maiden Tribute of Modern Babylon* (1885), as "part of a formative moment in the production of feminist sexual politics and of popular narratives of sexual danger" (pp. 2, 243). The "dense cultural grid" she constructs provides a framework for such sexual dangers as "same-sex relations, free love, commercialized sex, and nonfamilial" liaisons. And she connects the narratives arising out of these taboos to such basic issues as "work, life-style, reproductive strategies, fashion, and self-display" (p. 6). She also emphasizes the entry of middle-class women into the public sphere who spoke out for the first time about these sensitive issues and entered new spaces. Rejecting the traditional roles of "Dutiful Daughter" and "Womanly Woman," these "New Women" transgressed patriarchal boundaries and thereby earned such unflattering epithets as "unsexed women" and "Glorified Spinsters." By the tens of thousands, Walkowitz contends, they sallied forth from affluent houses in the West End to "wander free in interfamilial space" (p. 63) and perform good works in the slums.

Taking her title from Henry James's oxymoronic description of London as that "dreadfully delightful city," Walkowitz finds far more "dreadfuls" than "delights" to discuss, at least where women were concerned. While privileged and lustful men sought pleasure all over town, New Women were invading "homosocial space," contesting territory long considered a male preserve. Following Stead's metaphor, she depicts London as a Labyrinth filled with urban explorers, "slummers," and prostitutes along with a plethora of "male pests, cads, gents, and swells," who harassed respectable women in the streets and inside the "feminized" department stores of the West End. As a result there is little joy or mirth, let alone intergender peace, in Walkowitz's metropolis, which seems to have had much in common with James Thomson's deeply depressing poem, *City of Dreadful Night*. Gender tensions and the sexual abuse of women seem to have intensified once New Women began to demand higher education and careers—in short, entry into the public sphere and the rewards of power and knowledge. And yet the heroic efforts of "platform women" like Josephine Butler did little to erode the patriarchal double standard. Walkowitz even takes Butler to task for representing women in her propaganda efforts as innocent, helpless, and therefore in need of patriarchal protection.

With boldness and imagination Walkowitz ranges over the various conflicts that arose out of the challenge New Women posed to Old Men. Two central chapters discuss Stead's transformation of journalism and sexual politics by fusing melodrama and pornography in the *Maiden Tribute*. More than critical of his motives (not to mention morals), she invokes Simon Watney's concept of a "moral panic" to explain the impact of these articles on both men and women (p. 121). Playing the role of Minotaur rather than Theseus, Stead captivated countless readers with tales of lecherous "gentlemen" shopping for virgins. Prodded by Butler's crusade against child prostitution, Stead went beyond the conventions of journalism by buying a thirteen-year-old girl named Eliza (or Lily) for £5. But he paid a much steeper price for "playing with fire" after he was charged with abducting a minor and indecent assault. Relishing the role of hero/martyr, he was convicted and served time in prison, editing the paper from his cell. In Walkowitz's view, the *Maiden Tribute* did not just manifest Stead's "extraordinary rage against women," evidenced by the number of "evil" women who appeared in his story (p. 97). It inspired the guardians of social purity to renew their war on obscenity, it spurred feminists to confront the problem of prostitution and sexuality, and it persuaded M.P.s to raise the age of consent for females to sixteen.

Because versions of the last three chapters appeared in journals during the 1980s, they contain few surprises. Some additions notwithstanding, the final chapter on "Jack" recycles her article ("Jack the Ripper and the Myth of Male Violence," *Feminist*

Studies 8 [Fall 1982], 543–74) about the mythic and political elements of the Ripper story. After describing Whitechapel's confusion of empty and crowded streets or peaceful and threatening spaces, she delves into some of the discourses surrounding the murders, stressing the contribution of medical "experts" to the debate about sex crimes, "lust murder," and a "mad-doctor" killer. Weaving into her narrative the motif of medically approved violence against animals and women, she contends that "the dark media fantasy" produced by Fleet Street was the culminating event of "feminist sexual politics" in this decade. Apart from blaming the victims for their fate, many papers fanned the embers of anti-Semitism by focusing suspicion on working-class Jews. More controversial is her argument that the Ripper myth promoted a misogynistic backlash. Motivated by "deep-seated sexual antagonism," men began to impersonate "Jack" and threaten women with his kind of violence. In this way the vocabulary and iconography of the Ripper *mythos* "permeated the whole society" (p. 220) and made many women feel even more vulnerable.

Walkowitz ends with an epilogue that is ostensibly about the Yorkshire Ripper, who murdered at least thirteen women between 1975 and 1981 in a reign of sexually sadistic terror. But most of this section focuses on feminist responses to Peter Sutcliffe's deeds. Although applauding the way feminists organized patrols and protests against male violence, she also delves into the ideological divisions within British feminism during the 1980s. Her critique goes beyond the "blame-the-victim" school to include certain radical feminist positions on male sexuality. Writing as a moderate (and American) feminist, she draws connections between gender conflicts in the 1880s and 1980s and exhorts readers "not to play into the hands of the forces of political reaction," which seek "to turn feminist protest into a politics of repression" (p. 245).

No short review can possibly do justice to the insights in this richly conceptualized and annotated study. But there are also oversights and overinterpretations, which make some assertions more than moot. The synchronic approach adopted here foreshortens the historical perspective, thereby downplaying the continuities between the 1880s and what went before. The emphasis on "a new commercial landscape" and a "redefined public domain" in late-Victorian London, for example, gives the impression that respectable women rarely left their houses to perambulate, shop, or visit theaters and music halls before the 1860s (pp. 24–25, 45–46). Walkowitz also follows the Arnoldian tradition of giving Stead credit for inventing the New Journalism, neglecting the contributions of the mid-Victorian Sunday and evening press, as Joel Weiner and others have made clear. At one point she asserts that Jewish immigrants from Russia and Poland descended en masse on the East End in 1886 (p. 28). In fact, these refugees began arriving by the hundreds in the 1870s and then by the thousands after 1881. Calling the Irish in London "Anglo-Irish,"

moreover, is a solecism that will raise many a Hibernian hackle (p. 195).

Walkowitz tends to overplay the ease or comfort level of female charity workers while visiting the slums. Did they all feel so much more "at home" there than their male counterparts, and were they all so fearful of shopping in the West End? Convinced that "gentlemen of all sorts" prowled around Whitechapel in 1888 "looking for women to frighten," she insists that these "copycats" got away with these acts of intimidation (p. 218). There is no mention, however, of the exemplary sentences (a fortnight in prison with hard labor) passed by several magistrates on men found guilty of such acts in the hope of discouraging this dangerous behavior. She also maintains that the Ripper murders touched off a series of riots in the East End against not only Jews but the police and doctors as well (pp. 203, 214). But the absence of any casualty or arrest figures makes these riots somewhat chimerical. One of the more remarkable features of the Ripper saga was the absence in Whitechapel of the kind of violence that occurred around Trafalgar Square in 1886–87 despite all the tensions and fears of communal unrest.

More important, Walkowitz contends that middle-class male urban explorers were not only *voyeurs* but also *flâneurs*, who were able to cross "the divided spaces of the metropolis" and "experience the city as a whole." These men used their "privileged gaze" and fantasies to construct not only their own identity but also that of the Other. So far, so good. But Walkowitz's stereotypical *flâneur* includes male philanthropists and social investigators as well as those familiar loungers or idlers who were interested only in sexual (or spectatorial) gratification. Thus, Charles Booth, the pioneering sociologist, is reduced to a mere *flâneur* (p. 33), whose scopophilic desires outweighed any scientific interest in the poor (the thought of a *flâneur* producing the seventeen volumes of the *Life and Labour of the People of London* [1892–97] boggles the mind). One searches in vain for any *flâneuses* in this book. Instead, there are "intrepid" female philanthropists, who treated their clients with an empathy denied to men and who arrived at "new 'truths'" about the poor (pp. 54–56). One wonders what these truths were and whether or not these female philanthropists continued to feel "at home" in the East End after September 1888.

Walkowitz's emphasis on the role played by melodrama and pornography in narratives of sexual danger yields dividends. But only in passing does she acknowledge the tradition of sensationalism, which dates back to the first broadsheets and chapbooks sold on the streets. For many years newspapers had been sensationalizing everything from natural disasters, fires, and shipwrecks to crimes and executions. And the fashioning of such events into eye-catching feature stories was not confined to the penny press but could be found in elite morning papers. When deal-

ing with the Ripper murders, Walkowitz also neglects the important differences in the way newspapers handled the gruesome mutilations. There is no mention here of the curious omission from the *Pall Mall Gazette*, despite its reputation for pornographic indulgence, of all those excerpts from the autopsy reports that appeared in the *Times* and other "highbrow" papers.

Walkowitz finds an "uncanny resemblance" between the Ripper murders and "the typology of sexual crime foregrounded in the 'Maiden Tribute'" (p. 131). But this conflation of the two narratives ignores the fact that Stead's "Minotaurs" did not cut the throats and perform crude hysterectomies on their victims—a distinction that was not lost on readers in 1888. However deplorable their sexual appetites, these men roamed all over central London and did not confine their activities to the East End as did the Ripper. And there is one other important difference: no matter how shocking Stead's revelations in 1885, they did not incite the kind of panic or terror and anti-Semitism that ensued from the Ripper story. Walkowitz also gives short shrift to the law-and-order aspects of the murders. The failure of the police to catch the killer moved many to wonder how Scotland Yard could ever cope with the dangerous elements supposedly lurking in "the Abyss." Many Londoners called for a massive infusion of police into the East End and demanded that Chief Commissioner Warren resign. Like other editors, Stead played heavily on these fears. But Walkowitz does not mention the series of alarmist articles he published in October, entitled "The Police and the Criminals of London," which warned of a serious crime wave sweeping over all of London.

These problems notwithstanding, Walkowitz's book illuminates the contested terrain of London and shows how narratives of sexual danger contained messages instructing women to stay in their place and seek male protection. Empirical historians attuned to studying social relations rather than gender constructions will miss the narratives of class danger or conflict that loomed so large in this decade. But those who prefer a London filled with menacing Minotaurs, misogynistic journalists, patriarchal intellectuals, and fearful as well as intrepid women will find much inspiration in this richly conceptualized study. Despite their rather discrete quality, Walkowitz's chapters add up to a great leap forward from the old social history. Indeed, they make such vintage works as Helen Merrell Lynd's *England in the Eighteen-Eighties* (1945) and Herman Ausubel's *In Hard Times* (1960) seem prehistoric. Here is a book that not only deepens our understanding of sexual politics in the 1880s but also amply demonstrates the heuristic quality of feminist scholarship a century later.

L. PERRY CURTIS, JR.
Brown University

DAVID WILLIAM COHEN and E. S. ATIENO ODHIAMBO.
*Burying SM: The Politics of Knowledge and the Sociology of
 Power in Africa.* (Social History of Africa.) Portsmouth,
 N.H.: Heinemann or James Currey, London. 1992. Pp.
 159. Cloth \$45.00, paper \$19.95.

Silvano Milea (SM) Otieno, a distinguished and wealthy Kenyan barrister, died suddenly of a heart attack on December 20, 1986. For the next six months his embalmed corpse, stored at a Nairobi mortuary, became the focus of a bitter legal struggle that riveted the attention of an entire society. The case, followed in intricate detail in the press and by large crowds that gathered each day outside the courthouse, focused on a dispute between, on one side, his widow, Virginia Wambui Otieno, and, on the other side, his brother, Joash Ochieng' Ougo, and the Luo clan of Umira Kager over the proper site for SM's burial: his estate near Nairobi, close to his wife's Kikuyu people; or his birthplace in Nyalgunga in the Luo homeland in western Kenya.

After an exchange of injunctions and counter-injunctions by the lawyers John Khaminwa for Virginia Wambui and Richard Kwach for Joash Ochieng', which prevented the two sides from carrying out their respective plans for burying SM, the case was referred to the appeals court. The issue was whether SM, as a Luo, had to be buried according to Luo law and custom, as his brother and clan claimed; or, as an educated, cosmopolitan, Christian, and "modern" man who lived outside of customary law, as his wife and sons insisted, he should be buried in Nairobi according to his expressed wishes. Unfortunately, SM left no written instructions about his burial, and none of the several oral statements on the matter were recorded by his immediate family and other witnesses. The case also raised the issue of the Otieno's "modern" marriage across ethnic lines and the absolute right of Virginia Wambui to her late husband's property, including his remains, under statute and common law. This case opened an intense debate over the relative standing of the three legal codes used in Kenya: statutes, English common law, and the "customary law" of various ethnic communities. In May 1987 the case was finally resolved by the court of appeal in favor of Joash Ochieng' and the Umira Kager clan, and SM was laid to rest among his ancestors in Nyalgunga.

More than a sensational court case, burying SM was also a landmark event in the modern history, culture, and politics of Kenya. As David William Cohen and E. S. Atieno Odhiambo note, SM Otieno, in life the very embodiment of ideas of development in Kenya, became in death the focus for the revelation of the contradictions within concepts of "development," "tradition," and "modernity." The controversy over burying SM provided an opportunity to see "change in the making" and to understand it as constitutive rather than simply reflective of culture and history,

revealing the power of small events within large-scale social transformations (pp. 97–99). Cohen and Odhiambo therefore provide an important book about an important case, not only regarding the substance of their analysis but also in what it reveals of the strengths and limits of postmodernist methods in the study of history and politics.

In a densely packed text of 100 pages, the authors expound the divergent meanings of SM's life and death that collided in the Nairobi courtroom and became the focus of national debate. Cohen and Odhiambo show how, in the course of the debate, a variety of productions of history and culture took place, including their own text, which they see as part of the social process surrounding SM's death and burial. By viewing the production of history as organized social practice, they try to relate the diverse texts presented to the social relations of various actors and writers. Through this, we see the actual process of constructing national culture (pp. 17–20). At the end of the book they append a section of brief comments on both the case and their text by several colleagues, including Corinne Kratz, Martin Chanock, Peter Amuka, Sally Falk Moore, John Lonsdale, and Michael Fischer. This is a valuable addition that provides a basis for the critical evaluation of the book and significantly enhances its value for classroom debate.

Although they refer briefly to events outside the courtroom, Cohen and Odhiambo center their analysis on the transcript of the proceedings produced by the opposing counsels, their clients, and the witnesses. The deconstructive method is particularly illuminating in analyzing the content and omissions (silences) of the various discourses of the participants. This is especially revealing, for example, in their analysis of the representation of Luo culture and custom in the court. This representation relied heavily on the invocation of expert knowledge—ethnographic, legal, and historical—in the form of both written texts and oral testimony. Cohen and Odhiambo show, however, that both lawyers avoided calling on the wide range of modern scholarship on the Luo and other Kenyan cultures, relying instead on older anthropological texts (p. 79). In so doing, the lawyers effectively excluded evidence regarding significant changes in Luo burial practices in this century and accepted, each for their own reasons, an essentially colonial version of African culture as ancient and unchanging: Ochieng' and the Luo clan attempted to prove the authority, stability, and coercive power of custom, and how countryside burial sustained ancient Luo values, while Wambui and her

sons tried to depict Luo custom as primitive, crude, and brutal, and thus not applicable to a modern Christian like SM (pp. 59–67).

This book ends, however, just at the point when we might expect the most telling part of the analysis to begin. In their conclusion, Cohen and Odhiambo note that struggles for culture and history “are in fact struggles to constitute control over others by claiming authority over the power to inscribe the record of the past, the transcript of the person, and the description of culture.” But they leave the crucial “how,” “why,” and “to what effect” questions about this process deliberately unanswered because they are “committed to the development of productive questions as opposed to the anticipation of final answers and closures” (p. 95). It is not enough, however, to assert that history involves the confrontation of a multiplicity of voices, provide an analysis of those voices, and leave it at that. It is not surprising that a mode of analysis most strongly rooted in linguistics and literary criticism reduces the world to texts, but such relentless idealism quickly loses connection with the materiality of experience and the possibility of understanding the interaction of structure and agency in shaping history. Alas, postmodernist method stops at this point, just when the most compelling questions are posed, because in postmodernism all discourses, including that of the historian, are arbitrary constructions and none are, in any ultimate sense, true. Postmodernism leaves us with a theoretical sterility and epistemological relativism that begs the very questions that Cohen and Odhiambo propose to answer. As Martin Chanock notes in his commentary, the traditional issues of African history disappear in their text, along with the confidence that there is either “science” to provide a reliable analysis or a true story to be told. Instead, the focus is shifted to a concern with the writing of history itself, the authors’ own textuality. But this creates another and essentially foreign “postmodern” way of knowing that is irrelevant to the principal actors in the real drama of burying SM.

Historians and social scientists must deal with causes and consequences. They have a need—indeed, an obligation—to provide an explanation of the events they study, even if such explanations are never definitive; texts must be related to contexts. Some discourses are more equal than others, “officialized,” as Sally Falk Moore puts it, while others are dismissed, submerged, and suppressed in the political struggle among divergent voices that defines what

comes to count as “reality.” It is precisely this process that demands our attention. But it is characteristic of postmodern analysis to assume an affinity between the theory and the interests of the oppressed and to couple intense textual theorization with a neglect of context and its interplay with discursive factors. Cohen and Odhiambo are far too good as historians and know and care too much about Kenya to be wholly constrained by the limitations of postmodernist method, drawing attention to, but leaving unanalyzed, many of the most important aspects of the case. They express repeated concern for the silencing of Wambui, the dismissal of women’s voices, and the establishment of a dominant Luo male version of the story. They understand as well the particular significance of the case within the context of the building political crisis in Kenya over an increasingly repressive state and how the wishes of Kenya’s president, Daniel arap Moi, and his regime ultimately tipped the outcome of the trial. And they note the fates of the two counsels: Kwach was appointed by Moi to the appeals court, while Khaminwa was detained in July 1990 for his part in defending human rights and political pluralism (pp. 86–87). Finally, the authors are aware of how the case also touches on sensitive matters of class, especially the relations between wealthy urban elites and their poor rural clansmen, and how Kenyan youth reacted in songs and plays in secondary schools around the country in ways that suggest the emergence of a national culture beyond the ethnicities of Luo and Kikuyu and many others.

Such gestures, however, do not constitute adequate explanations. To discover those explanations requires a quite different theoretical perspective, notably a sociology of knowledge method such as that employed by Philip Corrigan and Derek Sayre in their study of the linkage between the development of the state and national culture in Britain (*The Great Arch: English State Formation as Cultural Revolution* [1985]). This would permit simultaneous consideration of the cognitive and the material, the cultural and the structural dimensions of experience. From this point of view, the dramatic confrontations and disputes, both in the courtroom and outside, over the burying of SM could be more fully understood as a key chapter in the hegemonic struggles shaping the development of Kenya’s culture and institutions.

BRUCE J. BERMAN
Queen’s University
Kingston, Ontario

GEORGE P. SHULTZ. *Turmoil and Triumph: My Years as Secretary of State*. (A Robert Stewart Book.) New York: Charles Scribner’s Sons. 1993. Pp. xiii, 1184. \$30.00.

George P. Shultz’s memoir of the Reagan administrations’ foreign policies is the most detailed, vivid,

outspoken, and reliable record we probably shall have of the 1980s until the documents are opened far too

many years from now. The major reason for the work's importance, aside from Shultz's own significant world view, is that the book largely rests on a "remorsefully precise record," as he calls it (p. xiii) of conversations, conferences, and Shultz's ruminations taken down in some 200 notebooks by the secretary of state's executive assistant, Charles Hill. This career foreign service officer participated in crucial meetings and undertook delicate diplomatic assignments from (and with) Shultz. Hill has a keen sense for historical context and a sensible commitment to preserving the historical record, as well as the gift of writing rapidly and legibly.

Explicitly proud of his Marine Corps service in World War II Pacific battles, Princeton education, success as a business school dean, president of the giant multinational Bechtel Corporation, and ability to serve President Richard Nixon as treasury secretary while not being tarnished by the Nixon administration's crimes and lies, Shultz was well prepared to help after Reagan fired his first secretary of state, Alexander Haig, in June 1982. Even this highly self-confident survivor of military, academic, corporate, and Washington battlefields, however, was astonished by the brutal struggle for the ear of the president and, at times, of Nancy Reagan.

Shultz's most worthy opponents were Defense Secretary Caspar Weinberger, CIA Director William Casey, White House Chief of Staff Donald Regan, and, above all, various National Security Council (NSC) members ranging from William Clark, Robert McFarlane, and John Poindexter at the top, down to Oliver North. All of these opponents had left, and most were driven from office by late 1987. Weinberger resigned after he understood that his defense budgets could not be unlimited and that, to his great concern, Shultz and Reagan intended to negotiate seriously with the new Soviet leader, Mikhail Gorbachev. Casey, leaving behind his policy disasters of the Iran-Contra scandal, died of a brain tumor. Clark, emulating Gary Cooper in dress and style, was perhaps the least knowledgeable U.S. foreign policy official in recent memory. Under Clark the NSC staff process was so bad that Shultz thought, "It's worse than a university" (p. 275). McFarlane, Shultz believes, was out of his depth as NSC adviser and tried to escape by finding a Kissinger-like secret mission that would reorient world politics. McFarlane instead found himself carrying a chocolate cake and a Bible on a disastrous mission to Iran that sought to free American hostages and transform U.S.-Iranian relations. John Poindexter's attempt to continue the Iran-Contra scheme, and cut Shultz off from the president, ended with Poindexter becoming the first admiral on active duty in U.S. history to invoke, repeatedly, the Fifth Amendment before a congressional committee. North is considerably more shadowy in *Turmoil and Triumph*. Shultz condemns his disdain for the Constitution, but, perhaps because the secretary of state saw Casey as the puppeteer behind

North's sometimes zealous nuttiness, he offers little analysis of this fellow Marine. By 1987 the NSC staff so hated Shultz and the State Department that at an annual picnic one former NSC adviser picked up a knife, stuck it in a cake, and proclaimed, "*That for the State Department.*" Hearing of the event, Shultz asked, "What is it about NSC advisers and cakes?" (p. 924).

Beneath the personalities and bureaucratic politics, however, lay fundamental policy differences. A thoughtful conservative, Shultz, supported by younger foreign service officers he (unlike some earlier and later secretaries of state) carefully brought into policy-making circles, usually correctly read Gorbachev and the Soviet crises. In 1979, Shultz had declared that "The Soviet system is incompetent and cannot survive" (p. 6). But the CIA, he believed, gave him little help in making the prophecy come true: "Our knowledge of the Kremlin was thin, and the CIA, I found, was usually wrong about it" (p. 507). In 1986, the agency's number two official, Robert Gates, flatly contradicted Shultz by declaring that the Soviets would not change: "The Soviet Union is a despotism that works" (p. 703). Shultz's best ally was the only one he needed. Reagan had it in his mind that the disappearance, rather than the deterrence, of nuclear weapons was to be preferred, and as Shultz repeatedly notes (sometimes, as in the Iran-Contra pages, to his own displeasure), when an idea lodged in the president's mind it could not be moved. Shultz consequently could override CIA and Defense Department opposition to negotiating the removal of short and intermediate-range nuclear weapons from Europe. Henry Kissinger warned him that such a deal with the Soviets "undoes forty years of NATO." Howard Phillips, chair of the Conservative Caucus, provided an interesting perspective on Shultz's conservatism by writing in an op-ed column of late 1987 that Reagan had been captured by a "pro-appeasement" group headed by the secretary of state. The president, Phillips believed, did not understand that "we are in a deadly, strategic end-game with the Soviet Union, militarily the most powerful regime in world history." Shultz comments, "What rubbish, but amazingly and distressingly, it had a following" (pp. 1007-08).

Other policy turns also generated bitter internal debate. In 1983, Shultz and Reagan ordered the invasion of Grenada, but only after overcoming intense opposition from the military Joint Chiefs who, "because of their Vietnam War experience," opposed the operation (p. 343). Shultz's struggles with the Pentagon (and the memories of Vietnam) over the deployment of force to accompany his diplomacy were nevertheless never-ending and highly frustrating. He repeatedly condemned Weinberger for wanting to spend billions on defense but not a cent on the actual use of force. The one occasion when Reagan and Shultz did not desire to employ military power was to remove Manuel Antonio Noriega from power

in Panama. As the State Department drove for a bargain with Noriega so he would leave voluntarily, Reagan and Shultz passionately argued (in Shultz's words) that "to get a person out of Panama" we should not have to "risk our kids' lives and theirs" (p. 1074). Their efforts were stopped first by Vice President George Bush and Treasury Secretary James Baker, who feared that any deal with a drug-smuggler like Noriega would wound Bush's presidential run in 1988, and then by Noriega's own refusal.

Another major turn occurred when the State Department finally led the effort to remove the highly damaged Philippine dictator, Ferdinand Marcos, from power in 1986. This time Shultz had to overcome the president's refusal to cut off Marcos, and when the dictator did depart, Shultz believed that his own relations with Reagan had perhaps been irretrievably harmed. The success also gives the memoirist, however, not only a chance to observe explicitly that some U.S. officials disliked certain authoritarian regimes but also an opportunity to attack implicitly the widely noted thesis of Jeanne Kirkpatrick that authoritarians deserved special support from Washington.

Certain important events are notable for their absence from this account, or for the small space devoted to them. Shultz's dislike for Noriega was real, but he helped the dictator considerably by attending the inauguration of Noriega's hand-picked, fraudulently elected presidential candidate in late 1984. The explanation for this trip is brief and lame. The sections on the Middle East are highly detailed, in part to show why U.S. policies had little lasting success, in part to explain why officials misread the key roles of Syria and Jordan. Shultz explains why he believed it wise to move closer to Iraq's Saddam Hussein in the early and mid-1980s, then argues that U.S. policy began to grow wary of the dictator. Elaine Sciolino has noted the book does not analyze Shultz's angry veto of an August 1988 State Department proposal to try to curb Iraq's growing power by lifting sanctions imposed on Iran ("In the Cockpit of the Free World," *New York Times Book Review* [May 16, 1993], 33).

In dealing with the bloody Central American crises in Nicaragua and El Salvador, Shultz observes how Reagan's military approach badly divided American politics, and how the State Department tried to cool the debate and improve the policy by having serious negotiations accompany the commitment of force. As Shultz angrily notes, this suggestion met strong opposition from the CIA and the White House. He is less satisfactory in explaining why he retained as his

top Latin American official Elliot Abrams, who became a lightning rod for criticism as U.S. policies failed and Abrams admittedly lied to Congress about those policies. Nor does Shultz deal at any length with how the illegal process developed of sending profits from Iranian arms sales to the Contras in Nicaragua; how he and other State Department officials tried to circumvent congressional bans on Contra aid by pleading with private, wealthy Americans, as well as with the Sultan of Brunei and Saudi officials, for money for the Contras; and why he went along with this tin-cup diplomacy, as Abrams termed it.

Theodore Draper's careful work (*A Very Thin Line* [1991], 553–54, 598; and "The Iran-Contra Secrets," *New York Review of Books* [May 27, 1993], 43–48), has raised a few questions about Shultz's role in the Iran-Contra scandal, but the secretary of state emerges as the only top U.S. official who consistently condemned the policy and tried to force Reagan to see the policy's contradictions. As a classic American conservative, Shultz values social order, willingly employs force to achieve that order, and strongly believes in late twentieth-century capitalism and the importance of markets unencumbered by government intervention—especially economic sanctions, which, although they have been a favorite of frustrated American officials since 1776, he has repeatedly and heatedly opposed.

A most revealing and significant event in these memoirs occurred in Moscow on April 14, 1987. Shultz gave Gorbachev a lecture on the technological "revolution" that was reshaping both the increasingly interconnected global marketplace and the world's military scene. New nations were becoming economic giants because their science and technology (unlike the Soviets) were "hitched to an incentive-based, market-oriented economic system." The most efficient manufacturing was global in scope, and because of the new "information age," markets reacted "almost instantly to a major event anywhere." No one nation, therefore, could again be so predominant in the financial—or military—field. Power was to be driven by knowledge, the "old categories" (such as Marx's belief in the separation of capital and labor) no longer held true, and the most important capital was "human capital" that moved freely amid the "information revolution" (pp. 892–93). Shultz had delivered a lengthy epitaph for the Soviet system and also the Cold War for which his memoir and Dean Acheson's *Present at the Creation* (1969) serve as book-ends.

WALTER LAFEVER
Cornell University

Reviews of Books

GENERAL

PETER BURKE, editor. *New Perspectives on Historical Writing*. University Park: Pennsylvania State University Press. 1992. Pp. 254. Cloth \$32.50, paper \$13.95.

Today history is so often written under the sign of the "new" that it has become increasingly difficult to tell the new from the old or the new-old from the new-new history. As Peter Burke's intelligent introduction to this collection of ten essays makes clear, the concept of a new history has been with us since James Harvey Robinson used it as a title in 1912. What was new history a decade ago—history from below, women's history, ethnic and nonwestern history—now seems outdated. It is striking that several of the contributors have chosen to deal with their subject in the past tense, almost as if it did not have a future. Jim Sharpe despairs that social history has had little impact on mainstream history, at least in Britain, while Joan Scott seems quite ready to consign her assigned subject—women's history—to the past in her eagerness to grapple with the critiques of the new gender history. Henk Wesseling provides a survey of what Europeans still insist on calling "overseas history," but only to invoke a new non-Eurocentric world history, to which he is yet unable to give shape or direction.

It is also interesting to note what the collection assigns to oblivion. It would appear that labor history, urban history, family history, psychohistory, and cliometrics have neither a past nor a future, for they make no appearance in this collection. Furthermore, the whole Marxist tradition is treated as if it were dead and buried, without so much as a memorial to mark its passing.

The overall impression is that much of what was new and promising in the 1960s and 1970s is either moribund or now bogged down in endless internal difficulties. As Giovanni Levi's fine essay on microhistory and Gwyn Prinz's insightful exploration of oral history reveal, new approaches have produced more problems than they have been able to resolve. The contributions by Ivan Gaskell on the history of images and Richard Tuck on the history of political thought will be of great interest to specialists, but the technicalities may frighten off the eager acolytes. More

welcoming are the contributions of Robert Darnton on the history of reading and Roy Porter's wholly original exploration of the possibilities of the history of the body. Both prefer to stress the possibilities rather than dwell on the problems, and these authors' enthusiasm is contagious. Equally compelling is Burke's concluding commentary on the much-vaunted return to narrative. He reminds us that the subject need not be the only measure of the new. New forms can regenerate old subjects, giving them a whole new life in the most unexpected ways. Burke suggests that the next generation of new-new history may not even be discoverable under the rubric of history, and that we should be looking elsewhere—to film, novels, and "thick descriptions"—to discover our future.

This is fascinating and exhilarating, but, by and large, the collection leaves one with the impression of fragmentation and isolation, not just of one new history from another, but of historians from history itself. What one misses here is the kind of engagement and commitment with larger issues that characterized new history both in Robinson's time, in the 1930s, and again in the 1960s and 1970s. The issues raised here are largely of academic interest; and it seems all the more remarkable that, at a moment in time when history is being made on such a vast scale and accelerated rate, historians should be so inward looking. No doubt all this history-making will produce its own version of new history, but its shape is not discernible in this collection. There are plenty of indicators here as to how the past may come to look as a new generation tackles the problems set forth in these essays, but it is unclear just what role historians will play in shaping rather than just narrating events.

JOHN R. GILLIS
Rutgers University,
New Brunswick

NORMAN SIMMS. *The Humming Tree: A Study in the History of Mentalities*. Urbana: University of Illinois Press. 1992. Pp. 271. \$44.95.

Social historians will want to try to read this text. But they should not approach it expecting the task to be

easy. Nor should they be drawn in by the subtitle, which they might take to suggest either an exploration of the concept and the practice of *mentalité*, a term associated with the often unarticulated and habitual beliefs and relations of everyday existence and promoted in the pages of the French journal *Annales*, or an account of the historiography of this notion of *mentalité*.

In spite of his book's subtitle, Norman Simms is not really interested in the history or the historiography of *mentalité*. Staple topics associated with this realm such as ritual and religion merit only passing reference, and historians who pioneered discussion of the concept—Marc Bloch, Lucien Febvre, Emmanuel Le Roy Ladurie, Natalie Zemon Davis—are barely mentioned, if acknowledged at all. But terms such as “englobement,” “nontext,” “textualizability,” “un-text,” and “black hole” have dozens of citations. One of the authors Simms cites most in his account is Lucien Goldmann. This suggests that Simms's book is more concerned with the method of approaching *mentalité* than with its actual history.

That method is unashamedly poststructuralist inasmuch as Simms's approach to *mentalité* is largely discourse-driven and stands in stark textual contrast to much of the *Annales* tradition's “cultural materialism” and methodological displacement of narrative through recourse to routinely generated and cumulative series of quantitative data. But Simms addresses *mentalité* with an ironic postmodernist textualism: since *mentalité* as it is constructed and then lived is so often about the unspoken and nonwritten, his method turns more on texts (which he notes is a convenient term for culture) that are unspeakable, unimaginable, and unthinkable. The history of *mentalité* is more about the inexpressible and the ways in which it meshes with “high culture” and seemingly known national systems to produce ways of thinking, being, and perceiving. To understand *mentalité*, Simms suggests, it is necessary to engage in a process of what he calls “englobement,” whereby the “black holes” of experience's “untexts” are brought into inclusion with the textuality of culture and civilization.

For Simms this process of recovery and reconstruction is best illuminated through the deconstruction of narrations, and storytelling—oral textuality—is his favored terrain. He draws examples from the distinct but, he implies, related mythological traditions of a wide array of storytelling practitioners, from Yaqui poets to ancient rabbis. This form is particularly germane to Simms's argument, for storytelling is so close to a text and yet so far from it that it illuminates nicely the text/untext relations he sees at the core of *mentalité*'s meaning. Not strictly narrative, storytelling is also mood and tone, a relationship of teller and listener that is both spoken and unspoken. It is also constantly remade, bound by words and the logic of a particular outcome, but persistently modified by presentation and reception. Texts and countertexts form and reform in relation to untexts and in the gaps and

holes the silences and invisibilities of these untexts are the beginnings and foundations of new possibilities, new textualities. Or so it seems to Simms.

Social historians will no doubt express skepticism—at times, open-mouthed bewilderment—about this book. They will no doubt wonder how they are to take this methodological injunction and actually apply it to studies premised on evidence and wary of its absence. But this book is a reflection of undeniable trends, and it should be read as such: it assumes the “linguistic turn” and then takes it one postmodernist step further, championing narration and narrative as well as displacing them; it acknowledges a set of anthropological sensitivities at the same time that it undercuts the conventional disciplinary convergence of history and anthropology by insisting on the constitution of meaning in ways somewhat foreign to both areas and more akin to contemporary developments in literary theory and criticism; it prefers to challenge and provoke, to tease rather than tell. These seem to be some of the signs of scholarship in our time. In that sense this mistitled book is indeed a humming tree, telling us not so much about *mentalité*, but about ourselves. Simms may not see the difference.

BRYAN D. PALMER
Queen's University
Kingston, Ontario

RICHARD WOLIN. *The Terms of Cultural Criticism: The Frankfurt School, Existentialism, Poststructuralism*. New York: Columbia University Press. 1992. Pp. xxv, 256. \$35.00.

In this collection of nine essays (five previously published) Richard Wolin forcefully articulates his participation in the increasingly prominent intellectual effort to domesticate and absorb the poststructuralist, deconstructionist critique of Western “logocentric” culture into a progressive and self-critical Enlightenment tradition of rational, secular, immanent cultural criticism. This “Hegelian” project of transformative negation and reaffirmation (of saving the baby while its bathwater drains away) has at least three interlocking foci: re-creating a rational basis for determining criteria of truth and meaning once belief in transcendent metaphysical grounding has collapsed; insisting on the possibility of maintaining generalizable (“human”) ethical norms through a process of intersubjective validation that avoids the pitfalls of imposed normalization; and constructing political and legal institutions that would sustain and nurture a rational community of autonomous subjects immune or at least resistant to the postmodern seductions of ecstatic merger or the anarchic play of power. In each area, Wolin's task is to differentiate the Enlightenment from its historical miscarriages, and thus rescue it from the misguided radical critique of our post-Nietzschean century.

Wolin pursues his project in three stages. His first three essays address the legacy of the Frankfurt school of critical theory. His rereading tries to rescue the rational, empirical, this-worldly dimensions of the works of the Frankfurt school in the interwar and war years from the trajectory (imposed in part by the postwar generation in self-justification of its own theoretical revisionism) toward complete historical pessimism, aesthetic redemption, and transcendental critique. In the second section of the book, texts from the existentialist tradition (Carl Schmitt, Martin Heidegger, Maurice Merleau-Ponty, Jean-Paul Sartre) are subjected to a similar immanent critique that differentiates the rational enlightened components of their work from the historical miscarriages of a nihilistic existentialism that threw itself into the arms of fascist politics and transcendent metaphysics.

All of this, however, seems like a prolegomenon to the final section, a vehement critique of poststructuralism (Richard Rorty, Michel Foucault, and Jacques Derrida) for its exaggerated and misguided critique of Enlightenment rationalism as a monolithic structure of domination, exclusion, and terroristic denial of the particular and the heterogeneous. The undifferentiated, totalizing, ultimately archaizing or transcendental cultural critique offered by poststructuralist texts, Wolin believes, threatens to give the ghost of Thrasymachus new life. Without rational criteria for truth and meaning, without universalizable ethical norms, "might" triumphs over or defines "right," and the ground for collective cultural resistance and critique, for the historical pursuit of both solidarity and autonomy, is dissolved.

Although Wolin's aim is to create a space for positive and constructive immanent criticism, his own critiques often seem peculiarly negative, abstract, and "transcendent." In part this is because of the framework of Wolin's critique. His positive affirmation of secular rationalism and humanism is constructed from arguments and texts not discussed in this book. The works of philosophers such as Charles Taylor, Hilary Putnam, and Alisdair MacIntyre, social theorists such as Jürgen Habermas, Richard Bernstein, and Thomas McCarthy, or historians such as Martin Jay thus appear as transcendent authorities grounding the negative critiques. Moreover, their absence from the substance of the book is also indicative of a certain lack of self-reflection on Wolin's part.

Despite the importance of self-reflective understanding in Wolin's conception of immanent criticism, he gives the reader little sense of the specific historical perspective and situation that frames his own critiques. Also problematic in light of the book's commitment to immanent historical criticism is its tendency to equate "history" with the arguments of philosophers. One gets no sense from Wolin's essays, for example, that the poststructuralist critique of Western metaphysics might be connected in complex and differentiated ways to historical transformations in sexual and gender relations, or to redefinitions of

ethnic, class, and national identification in the post-colonial era.

JOHN E. TOEWS
University of Washington

JACK GOODY. *The Oriental, the Ancient and the Primitive: Systems of Marriage and the Family in the Pre-Industrial Societies of Eurasia*. (Studies in Literacy, Family, Culture and the State.) New York: Cambridge University Press. 1990. Pp. xix, 542.

Jack Goody is one of the world's foremost anthropologists, and his works have ranged across a wide expanse of topics and areas of the world. The present work is not a new line of inquiry for him, but a further development of a problem he has been working on for more than two decades: what are the major axes of differentiation of family systems around the world, and what are the bases of this differentiation? Goody's answer to this question was already well developed in his *Bridewealth and Dowry* (with J. Tambiah, 1973). There the authors argued that qualitative differences existed between the family patterns of subsaharan Africa and Eurasia, and that in relation to this major contrast, the differences across all of Eurasia were less important (perhaps more quantitatively than qualitatively). In the present work, Goody is primarily marshaling increased evidence on the Eurasian side of this comparison by dealing with a large array of Eurasian systems, ranging across time and space from ancient Egypt and Greece through pre-modern China, India, and Tibet, and contemporary societies in the Near East, Albania, Taiwan, and elsewhere.

Although this is an oversimplification of a complex argument, Goody contends that Eurasian preindustrial societies were characterized by intensive plow agriculture and complex political and social class hierarchies. These traits were related to distinctive family patterns. At the core of Eurasian family systems, contends Goody, were various forms of what he calls "diverging devolution." This unwieldy term refers to customs allowing women as well as men to receive a share of family property, whether in the form of inheritance, a dowry, or what he calls an "indirect dowry." The latter custom has often been misleadingly labeled bride price, but it is nothing of the kind. It involves the family of the groom presenting gifts to the family of the bride, who then use those gifts as part of their effort to provide her with a dowry that she takes with her into the marriage. Whatever the specific form of this "women's property complex," the result is that women have a central role and often considerable influence in how family property is managed and transmitted. The logical link back to the complex stratification system of these societies involves the fact that families use property endowments to preserve and enhance the status of their daughters (through marriage to a high-status

male, for example) as well as their sons. In sub-Saharan African societies, in contrast, intensive agriculture and complex stratification systems were absent, as was diverging devolution. Instead marriage finance often involved true bridewealth exchanges, with gifts moving around from family to family, with no endowment of daughters with property that would be merged with that of their husbands into a "conjugal fund" (as in Eurasia).

What has Goody added to his argument and evidence with the current work? Primarily he has introduced here evidence on a wide range of Eurasian societies to bolster his case. In the process, he also presents interesting information on regional and social-class variations in several of the regions he discusses. He uses these to show, in part, that there is a recurring tendency for higher classes to emphasize direct dowries, arranged marriages, and restrictions on divorce and remarriage, while lower classes are more likely to be characterized by indirect dowries and greater freedom of mate choice, divorce, and remarriage. He implies that some past contrasts drawn between European and Asian family systems have been based on misleading comparisons of the "apples" of ordinary European families with the "oranges" of elite families in Asia.

Goody also uses this volume to demonstrate his prodigious reading of ethnographic and historical works on a wide range of societies, a reading that enables him to present generally well-informed discussions of topics normally discussed only by regional specialists. Along the way a large number of tangents are pursued—discussions of polyandry in Tibet, Nayar "non-marriage" in India, and brother-sister incest in ancient Egypt, to mention only a few. Some of these discussions range so far from the central questions of this book and add so relatively little to the previous literature on these topics that some readers will wish that Cambridge University Press had departed from its usual hands-off editorial policy. Goody's arguments might have been better served by a briefer and more tightly focused discussion that avoided pursuing every possible interesting tangent.

In the end, how convincing is Goody's central set of claims? Frankly, I feel he presented his case more effectively in *Bridewealth and Dowry* twenty years ago. One element added in the present volume is a more polemical treatment of opposing points of view, and this treatment weakens the force of his arguments. Goody quite correctly criticizes earlier authors who drew too sharp and ethnocentric a divide between the supposedly enlightened family patterns of the West, with its relative freedom for women and youth autonomy, and the "primitive" East, with its patriarchal family forms and quasi-purchase and incorporation of brides from other families. In the process, however, he largely ignores more recent comparative treatments, such as those by John Hajnal ("Two Kinds of Preindustrial Household Formation System," *Population and Development Review*, 8 [1982],

449–94) and Alan MacFarlane (*Marriage and Love in England: Modes of Reproduction, 1300–1840* [1986]), that document genuine differences in family forms in premodern times between Western European societies and Asian ones. Goody portrays opposing arguments in straw-man form, and in the process he goes too far in claiming that there was not much difference between family forms in major regions of Eurasia.

I submit that this is not an instance in which one side is right and the other is wrong. Rather, Goody and his opponents both have important points to make. The family patterns of sub-Saharan Africa and Eurasia in general are qualitatively very different, as Goody claims. Within the major regions of preindustrial Eurasia, however, there were also major contrasts in family patterns (roughly between northwest Europe and other parts of Eurasia) that could also be considered qualitative, rather than simply quantitative (contra Goody). Stressing one set of differences does not require that one belittle the other, as Goody tends to do.

Despite the criticisms offered here, this book is not a work to be dismissed or ignored. Even those who do not subscribe entirely to Goody's position will find much to provoke their thoughts in this work. Even specialists on the particular areas he deals with will find that his outsider's perspective and broad erudition produce many comments and insights that may alter the way they think about the society on which they specialize. Goody, even when not at his most persuasive, is always well worth reading.

MARTIN KING WHYTE
University of Michigan,
Ann Arbor

WOLF LEPENIES. *Melancholy and Society*. Translated by JEREMY GAINES and DORIS JONES. Foreword by JUDITH N. SHKLAR. Cambridge: Harvard University Press. 1992. Pp. xvi, 253. \$39.95.

Wolf Lepenies is a distinguished historian and sociologist, but one may question whether the appearance in translation of his first book, *Melancholie und Gesellschaft* (1969), over twenty years after its initial publication, serves much purpose. Deploying a dazzling parade of learning in the traditional German academic manner, Lepenies takes it on himself to write the social history of boredom, juxtaposing ennui against its opposite yet double, utopianism. In this endeavor, Lepenies obviously aspired to create a lofty work of cultural history in the tradition that extends from Karl Mannheim to Michel Foucault, an intellectual heritage to which he makes devoted reference. But the outcome is a volume encumbered with scholarly allusions in which the parts are greater than the whole.

Despite Lepenies's penetrating insights, it is not convincingly demonstrated that tedium truly has an integral history of its own, certainly not one ade-

quately elucidated by Lepenies's rather rough-hewn contextualization in terms of the vacuity of courtly ritual and the impotence of bourgeois alienation. We are conducted on an itinerary of dejection that leads from Robert Burton in seventeenth-century Oxford (oddly, on page 11, Burton's *magnum opus* is called an eighteenth-century work) through the palaces of Louis XIII and the middle classes of the German Enlightenment, up to Marcel Proust and Sigmund Freud. But a fatal arbitrariness attends the inclusions and omissions. Eighteenth-century London was widely seen as the Mecca of depression, stimulating the labeling of melancholy as *The English Malady* in Dr. George Cheyne's best seller of that name (1733). But these themes receive not even a mention here. And why only the most glancing references to the Russia of Mikhail Lermontov or Anton Chekhov?

A cascade of quotations from the giants of Germanic scholarship, interspersed with allusions to Søren Kierkegaard, Friedrich Nietzsche, and Albert Camus, here serve in place of sustained analysis. And many of Lepenies's interpretations now seem distinctly dated. His listless sleepwalking Central European burghers hardly ring true in light of far more robust (and convincingly documented) interpretations of the same cultural elite in Richard van Dülmen's *Die Gesellschaft der Aufklärer* (1986), translated as *The Society of the Enlightenment* (1992). After two decades of "history of mentalités," it is peculiar that Lepenies has not chosen to furnish a revised edition or at least add a new preface.

Following the bad habits of orthodox Germanic scholarship, Lepenies's text is also frequently obscure and abstract. Paragraph after paragraph of murky prose induces a certain boredom in the reader: "Utopias such as Orwell's [1984] . . . play off the partially realized utopia against the envisioned utopia. They overlook the fact that reality is hardly necessary in order to perceive the utopian thrust involved, which comes to light in the prohibitive rules to which the utopians must submit; the planners of utopia forbid them to be bored, owing to an awareness that boredom is the result of the total plan; the planners categorically demand demonstrative happiness because they know that the assertion of providing the highest good for all can be proven only in formal terms—without preventing the individual from trying to withdraw into private spaces in which he can at least indulge in unplanned and unmeasured mourning" (p. 95). Author and translators seem equally to blame for this.

At bottom, two weaknesses render Lepenies's undertaking more striking in intention than successful in execution. On the one hand, the work suffers from a lack of conceptual precision. The terms melancholy, monotony, *Weltschmerz*, boredom, ennui, apathy, and neurasthenia are casually bandied around and all too readily fuse with each other, without sufficient regard for nuance and the different functions they have served (the gendered nature of the language of mel-

ancholy is almost overlooked). On the other hand, there is a lack of definition regarding what a history of melancholy might be expected to reveal or resolve. All too often, Lepenies rests content with bombarding his readers with salvos of disparate references in paragraphs that can leap from Galen via Schopenhauer to the Winnebagos. Such discussions, while attesting his erudition, contribute little to our illumination.

Finally, the production standards of this volume fall well below those customarily set by Harvard University Press. Note, for instance, the glaring typographical monstrosity on page 55.

ROY PORTER

Wellcome Institute for the History of Medicine

JAMES R. GIBSON. *Otter Skins, Boston Ships, and China Goods: The Maritime Fur Trade of the Northwest Coast, 1785–1841*. Seattle: University of Washington Press. 1992. Pp. xiii, 422. \$45.00.

This book by James R. Gibson represents the capstone of his more than twenty-five years of industrious exploration of the history of the Northwest Coast. Through monographs and articles Gibson has worked to broaden historians' appreciation of the complex political, economic, and social impact of the fur trade on the North Pacific basin. The book is the synthesis that pulls together Gibson's years of study, and as such it is a book that has long been needed and was much anticipated.

Gibson's narrative is comprehensive. Although the focus is on the Northwest Coast of North America and its native inhabitants, Gibson skillfully integrates the trade's impact on the entire Pacific basin. Not surprisingly, Gibson concludes that of the diverse peoples brought together by the Northwest Coast fur trade the Indians were the most seriously affected. Unlike in interior regions, however, European merchants failed to establish numerous trading posts on the coast. Without these nodes of Western influence on the frontier, European colonization, missionary work, and political control were retarded. Most of the trade was conducted by roaming sailing ships, yet European diseases, particularly smallpox and measles, devastated the independent-minded coastal peoples. The fur trade also increased the importance of slavery among the powerful and well-supplied coastal Indians. Slave raiding by well-supplied Indians helped to virtually depopulate areas inhabited by more isolated peoples. Out of self-defense and economic self-interest, inland Indians gradually adopted the culture of their powerful coastal neighbors. Gibson also details the way Euroamerican technology enhanced Indian hunting and stimulated monumental art. "For the Northwest Coast Indians," he writes, "the trade was neither just destructive ('looting') nor just constructive ('enriching') but both" (p. 269).

Although Gibson's treatment of the Northwest

Coast Indians is insightful and dispassionate, his book is unique for its appreciation of the fur trade's considerable impact on the Hawaiian Islands and China. American fur trade vessels used Honolulu as a base of supply. Their trade helped King Kamehameha I unify the Hawaiian Islands into a single kingdom, but it also introduced tobacco, heavy drinking, and epidemic disease to the islands. Ironically, the Hawaiians were changed more by the Northwest fur trade than the Indians of the coast. Sea otter furs had a much less dramatic effect on China, although Gibson establishes the fur trade as a part of a larger process that gradually forced the Manchus into the capitalist world system. Conversely, Boston, the home port of the majority of the maritime fur traders, was rejuvenated by the highly profitable trade that brought the capital necessary to seed industrialization.

Gibson's detailed presentation is empowered by thorough research, including Russian language sources. Cyrillic documents give the work a broader vision than previous studies; ship logs and journals by American and British traders, however, form the book's foundation. Gibson's narrative style is straightforward, if not literary, and it is refreshingly free of the jargon in which historical geographers sometimes encrust their findings. Gibson has written the definitive book on an important phase of North American frontier history.

THEODORE J. KARAMANSKI
Loyola University of Chicago

LOWELL DITTMER. *Sino-Soviet Normalization and Its International Implications, 1945–1990*. (Jackson School Publications in International Studies.) Seattle: University of Washington Press. 1992. Pp. viii, 373. \$35.00.

A brief review cannot do justice to this complex book. Lowell Dittmer's avowed purpose is to attempt to "reorient the discipline" to take account of the recent Sino-Soviet reconciliation: "its motives, dynamics, international ramifications, and possible drawbacks or limits" (p. 11). Dittmer is to be commended for pulling together in topical and chronological form a substantial amount of newly available data on the subject that will be useful to practitioners in the field.

Unfortunately, the information is not presented in a way to increase our understanding of the course of Sino-Soviet relations since 1945. Indeed, attempting to analyze the Sino-Soviet relationship as a self-contained set is (as Dittmer himself realizes) part of the problem, for the relationship is only a piece in each country's larger strategic set. In other words, their policies toward each other cannot be explained solely in terms of their relationship. Dittmer explicitly, but only partially, recognizes the inadequacy of the Sino-Soviet construct by devoting roughly half of his book to a discussion of the strategic triangle: the Soviet Union, the People's Republic of China, and the United States.

Dittmer devotes eight of his sixteen chapters to a discussion of the various permutations of the strategic triangle. Thus, he views the immediate postwar period as a *ménage à trois*, the 1950s as a Sino-Soviet marriage, the 1960s as a unit-veto triangle, the 1969–76 period as a romantic triangle, followed by a Sino-American marriage until 1981, a Sinocentric romantic triangle through 1985, and a *ménage à trois* again from 1986 through 1990. He concludes with a questioning note about the end of the strategic triangle.

The concept of a triangle is better than the duality of the Sino-Soviet relationship because it brings into the analytical framework the most important actor affecting Russia and China, the United States. Unfortunately, Dittmer does not apply the concept of the strategic triangle systematically to all sides. The point of view throughout the book is of China, which results in a distorted discussion of the triangle.

Fundamentally, the weakness of the book is the substitution of concept and jargon for analysis of people, process, and policy. There is a lack of explanation of why things happened when and the way they did. And it seems to me that the reason the book lacks such explanation is because the author fails to identify the strategies in play, without which there can be no understanding of why things happened but instead only laundry lists.

Thus, even though China is at the center of the book, there is no discussion of Chinese strategy, let alone Soviet or American strategy. Even though Dittmer notes that it was the Chinese leadership that took the initiative in reshaping the Sino-Soviet relationship throughout—from initiating the conflict to resolving it—the reader is left at the end with no inkling of why Chinese leaders made the decisions they made, why the conflict happened, why it ended, or indeed why anything occurred at all. The only attempt Dittmer makes at an explanation of the Sino-Soviet split is to assert the dubious notion that it was a function of China's search for national identity. Thankfully, he does not go so far as to say that Sino-Russian rapprochement is a function of its discovery.

RICHARD C. THORNTON
George Washington University

JEFFREY N. WASSERSTROM and ELIZABETH J. PERRY, editors. *Popular Protest and Political Culture in Modern China: Learning from 1989*. (Politics in Asia and the Pacific: Interdisciplinary Perspectives.) Boulder, Colo.: Westview. 1992. Pp. xi, 300. Cloth \$49.50, paper \$19.50.

This collection of essays edited by Jeffrey N. Wasserstrom and Elizabeth J. Perry delivers both more and less than it promises. More comes in two forms, beginning with its geographical reach, for the book is not limited to China. The question that animates much of the volume is why in 1989 East European

regimes collapsed in the face of protest movements while the Chinese party-state did not. Accordingly, the book contains a substantial essay on Eastern Europe (by Daniel Chirot), a region to which some of the other chapters are also addressed. Second, while 1989 is the point of departure for the book and the pivotal year around which its major themes revolve, historical depth is evident throughout the volume. The editors have assembled a formidable array of scholarly talent representing at least three or four disciplines, and all of the contributors pay serious attention to fundamental historical issues of continuity and change. There is something substantial to be learned from every chapter, and many of them raise fruitfully discomfiting questions about how historians think.

Although the book's dust jacket disclaims any intention to present a unified interpretation of 1989, attempts in the essays to explain the different outcomes in China and Eastern Europe imply something close to such an interpretation. Nevertheless it is the Chinese protest movement that gives the book its focus. According to Elizabeth Perry's introduction, scholars of contemporary China have pursued lines of inquiry that repeatedly (not only in 1989) left them poorly equipped to understand Chinese popular protest. This book therefore intends to pioneer a new approach to the study of political culture, one that will more fully explain political change in cultural terms. As Perry describes "the new political culture" and the "neoculturalist perspective" (pp. 4-6), it seemed quite familiar, and all the more so as she goes on to say that the central theme of the volume is "the importance of innovation within tradition" (p. 10). Nothing she says in this chapter would have surprised Tocqueville or even Voltaire.

The chapters that follow, however, are full of rewards and, despite the fact that several of them have previously been published elsewhere, surprises as well. The essay by Joseph W. Esherick and Jeffrey N. Wasserstrom, for example, will be familiar to readers of *The Journal of Asian Studies*, but it is so rich and nuanced that it repays another reading. (Readers of the original article will also be interested in the brief postscript that addresses questions raised by readers of that journal.) This version also profits from its inclusion in an anthology that treats related matters from different angles. Thus, its discussion of the Chinese student movement as a form of street theater, a form that testifies to the power of symbols and ritual in Chinese politics, is nicely complemented and enriched by several other chapters: anthropologist James L. Watson comments perceptively on the continuing re-creation of Chinese identity, with particular reference to China's oral traditions and rituals; John Israel provides a historical perspective on Chinese student movements going back to the late nineteenth century; Vera Schwarcz treats history and memory from the unique standpoint of two historical conferences she attended in China while the demon-

strations of spring 1989 were in progress; and Wasserstrom returns in the book's final chapter to comment on the blurred lines between history and myth, employing to stimulating advantage Northrop Frye's views on tragedy and romance. These and other essays in the book touch on and react to each other to an extent that is seldom seen in such collections.

For nonspecialists of China, and perhaps for some specialists too, the biggest surprise may be the picture the essays give of the student movement's lack of democracy. If there is anything close to a consensus in the book, apart from the comparison with Eastern Europe, it is that the students, and most educated Chinese, were too imbued with old elitist attitudes and too institutionally linked to the party-state to challenge the regime effectively. The attitudes in question concern deference to authority and, perhaps even more tellingly, the students' claims to high social status and their disdain for workers, peasants, and entrepreneurs. Attitudes of this sort contributed materially to the students' failure to form alliances with groups that shared their aims. More fundamentally, several authors contend, such attitudes have long inhibited the development in China of the kind of "civil society" that the authors generally hold to have been vital to the success of the protests in Eastern Europe. This case is made most powerfully by Timothy Cheek and Elizabeth Perry, but others add to the brief, and Lee Feigon weighs in with the complementary point that the student movement was marred by traditional biases against women. Stephen R. MacKinnon, although his focus is elsewhere, enriches the discussion with a short but penetrating look at one critically important segment of the intelligentsia, the Chinese press, noting that it has long been engaged in a struggle for freedom that may yet contribute to the creation of a civil society in China.

The analysis of China and Eastern Europe that runs through the book is highly insightful as far as it goes, and it suggests that the neoculturalist perspective is a useful analytical tool to add to our kit. But one wonders whether its utility extends beyond the study of popular movements. We cannot compare outcomes in China and Eastern Europe without also considering the actions of those who held power, especially since leaders in Eastern Europe explicitly rejected "the Chinese solution." Does the neoculturalist perspective tell us anything about how culture shaped the Chinese Politburo's decision to crush the protest by force? Can one of the major differences from Eastern Europe possibly be that China came first? Beyond such speculation is a more fundamental point: to understand why regimes fail and collapse, we must be clear about what kept them standing and what led them to crisis and beyond it. East European regimes stood because of Soviet power, and crises in Eastern Europe turned into revolutionary situations to some degree (I would say to a large degree) because of the actions, or rather nonaction, of the Soviet Union. For Daniel Chirot to state that, without

Gorbachev's reaction to the crisis, what happened in 1989 would have occurred in the 1990s (p. 231) is not only speculative but also skirts the key issue. He attributes the collapse of East European regimes mainly to "utter moral rot" (p. 232), but such rot existed from the earliest days and was not absent in China; when and why did it become utter enough to bring about collapse? And why did the growth of civil society reach a flash point all across Eastern Europe at the same time? Can the new political culture tell us about immediate causes as well as long-term forces? To historians, the fact that things happen sooner rather than later is not as important as it is to the people who live under harsh regimes, but it matters a great deal. In any case, it is probably too soon to speak of outcomes, especially with so many former Communists still holding power in Eastern Europe.

As for China, its main difference from Eastern Europe in 1989 may have been that a truly revolutionary situation never quite developed, at least not outside of Beijing and perhaps not even there. This weighty and fascinating book takes us a good part of the way toward an understanding of why this was so, but by concentrating its analysis of 1989 so heavily on the student movement it has given us a rather skewed perspective on the political confrontation of that spring. To see only one party to a political struggle in action is to hear the sound of one fist flailing.

Perhaps the notion of political culture that informs the book is just a bit too culture-bound. It may also be too culture-bound (in the more usual sense of that term) in its tendency to evaluate Chinese democracy by Western standards and somewhat ahistorically. But if the book overreaches in some respects it is a mark of how much it aims to achieve. The volume is a major contribution to knowledge and an exceptionally stimulating collection of quality essays.

MICHAEL GASSTER
Rutgers University,
New Brunswick

BRUCE CUMINGS. *War and Television*. New York: Verso. 1992. Pp. viii, 309. \$24.95.

Bruce Cumings is a historian of the Korean War, an avid television fan (he tells us), and the chief historian for Thames television's documentary *Korea: The Unknown War*, which appeared in Americanized form on PBS. The book culminates in an account of his experiences in the making and unmaking of the documentary, and he is strongly critical of WGBH, the PBS station in Boston which reworked it for American television, for bowing to political pressures and diminishing its accuracy and impact. Throughout the book Cumings gives his reflections on television, the documentary genre, and the television wars in Vietnam and the Persian Gulf.

The book is something of a polyglot. It is partly a participant's account of office politics at Thames and

WGBH, partly a travelogue (spiced with quotations from Baudrillard) recounting Cumings's trip to gather interviews in North Korea, partly a collection of personal observations about television, and partly an analysis of television as a means for representing history both as it unfolds and as past.

It is in this last mode that Cumings is at his best. As he puts it, the book is "about how we think: especially, why we have been led to think the camera's eye is better than the mind's eye, and how the archival photo, film or document interacts with the repository of the mind's eye: memory . . . about the *point d'honneur* of both the professional historian and the television medium: objectivity" (p. 3). Cumings makes extremely interesting if not fully developed points about what still and moving images can tell us about the past, about the value of eyewitness accounts and how they should be used in making documentaries, about the "emotivism" of television, which converts points of view into feelings, leaving them beyond criticism yet intellectually toothless, and about the relation of objectivity to historical authorship. He argues that an authentic point of view is essential to real objectivity. This puts him at odds with the normal practice of television journalism, which is to deny and conceal authorship.

In its other modes the book is more uneven. The account of the making of *The Unknown War*, which takes up about half the book, is certainly valuable, although I felt it got bogged down at times in the author's personal conflicts with other participants. Like the accounts that were the raw material for the documentary itself, of course, this is one participant's view, and as such inevitably limited. I also found the general discussions of television maddeningly loose in parts, often simply the author's personal reactions to things he had watched, with little concern about evidence or context (normally scrupulous scholars often become loose in this way when they write about TV; maybe the emotivism rubs off). To give a small example, Cumings claims that after Jimmy Carter was seen "on all three networks" fending off a rabbit with an oar, "no matter what he did, he was finished" (p. 36). Did the recession of 1980 and the Iran hostage crisis not have something to do with Carter's losing a close election?

DANIEL C. HALLIN
University of California,
San Diego

ANCIENT

MOTT T. GREENE. *Natural Knowledge in Preclassical Antiquity*. Baltimore: Johns Hopkins University Press. 1992. Pp. xix, 182. \$24.95.

In seven essays Mott T. Greene demonstrates the importance of applying a knowledge of natural science to the study of ancient history. Chronologically the essays range from prehistory through Plato and

geographically from the caves of southern Spain and France to ancient India.

I am torn because of a conflict between duty to the profession and consideration for the reader. There is the duty to summarize the conclusions but, in revealing those detections, the reader of this review would be robbed of the extraordinary pleasure of being led by Greene's clever logic in reconstructing the facts. A murder mystery reviewer should not reveal the killer. This scientist-author assembles the facts of each case, reviews the findings of others, reveals the details left unexplained, and reassembles the data into a logical explanation based on understanding of science and a respect for the law and order of scholarship. I am tempted to suggest that one who intends to read this book should read no more of this review.

The first essay, on prehistory, is a caution that in our search for beginnings we reconstruct achievements on the basis of preserved findings. Greene argues that the soft artifacts that were lost would give us a picture of discoveries far more remote in time than present scholarship's claims of "firsts." The next essay is less iconoclastic because it explains better the accepted wisdom that the Egyptians were poor abstract mathematicians. By showing that fractions were the product of scribal devices for craft and construction activities, the author surmises that the craft-based, practical arts "forestalled mathematical invention" (p. 44).

The third and fourth essays are related. Hesiod's descriptions of battles between Zeus and the Titans and Typhoeus show how volcanic activity might be described as sequences of events, sounds, and effects. Hesiod is sufficiently accurate in locating specific volcanoes and eruptions that his work could have been employed in antiquity as places for travelers and colonists to avoid. Greene counsels classicists "never to accept textual emendations of such passages made on purely philological grounds" (p. 72). Similarly, the stories of the Cyclopes, sometimes described as one-eyed giants, are descriptions of volcanic activities by those who did not see "humans as 'we' and nature as 'it'" (p. 86).

The historic or legendary Thales's alleged assistance to Croesus's army is shown to be the act of an engineer. According to the incredible story, the army "crossed the river" by having the river bed diverted from the front to the army's rear. Greene identifies the river as the Haly. In the general location of the river crossing, the Haly meanders in ox-bows that enabled a clever observer to cut a silted, restricted channel thus changing the river's course with relatively little effort. No longer is the story incredible.

The sixth puzzle provides an identification of the elusive soma, the herbal brew of some plant that is described in Vedic and Avestan works, and possibly in some Western rituals. Recently R. Gordon Wasson proposed that a mushroom was the ancient's soma, but Greene points to finger millet. Rather than using a botanical approach, Greene employs the pharma-

ceutical characteristics named in the sources to identify lysergic acid, a product of fungi, probably as found on *Eleusine coracana* or finger millet. Finally, Greene analyzes Plato's use of myth, especially in *Phaedrus* when myth is rhetorically told in our terms of "physiological psychology." To explain his views on the soul's transmigration, Plato employs optical and psychological imagery to reveal inward truth. The author achieves his purpose: we learn that the ancients' wisdom can better be understood by modern science.

JOHN M. RIDDLE
North Carolina State University

FIKRET YEGÜL. *Baths and Bathing in Classical Antiquity*. Cambridge: MIT Press or The Architectural History Foundation, New York. 1992. Pp. ix, 501. \$65.00.

The famous, if cynical, slogan, "bread and the circus," defines some of the palliatives freely given to the Roman people to keep them quiet. The slogan, however, omits an equally important item, the baths and the public bathing associated with them, that entered no less deeply into the fabric of Roman urban life and probably had a greater physical presence in the Roman city than any other institutionalized activity. Public bathing was a popular cultural institution, a major constituent of the Roman concept of *humanitas*, the esteemed quality that distinguished civilized men from savages and made life much more than merely bearable. Public bathing appealed to all levels of society and to both sexes. Because this activity was housed and serviced in large structures, the Roman baths provided opportunities for commercial exploitation by private entrepreneurs in the entertainment business, for the relatively free mixing of social classes, for politically useful expressions of largess on the part of important or would-be important public figures, including the emperor, and for original developments in Roman architecture and engineering.

Public bathing took place either in private facilities, called *balnea*, which were operated for profit, or in public baths, *thermae*, owned and maintained by public organizations, ranging from the municipality and its officers to the imperial treasury. Whether or not the facility was private or public, Roman bathing was a public activity, involving large numbers of persons who were getting wet, swimming, sweating, exercising, and, above all, socializing with friends. All these activities were contained within the precinct walls of the bath establishment, some of them like the Baths of Caracalla in Rome of vast extent, and were, thus, bodily removed from the affairs and cares of daily life.

Despite the acknowledged importance of the Roman baths in the history of ancient architecture and the magnificence of their standing ruins, little work of a synthetic nature has been done on them since Daniel Krencker's magisterial study of the imperial

baths at Trier (*Die Trierer Kaiserthermen* [1929]), although piecemeal studies of their typology and construction and of individual buildings abound. With the exception of K. Dunbabin's and J. DeLaine's articles in the late 1980s, little has been done on the institution of public bathing in the Roman world despite its centrality to the exercise of leisure (*otium*), its close association with the theater, arena, and circus as the principal loci for the pursuit of leisure by the urban masses, and its great demand on the financial and water resources of the state. But now two major studies have appeared. The first, by Inge Nielsen (*Thermae et Balnea: The Architecture and Cultural History of Roman Public Baths* [1990]), developed the institutional complexity of the social forces involved in the creation, support, and use of the baths. The second study, and the subject of this review, is by Fikret Yegül, an experienced Roman architectural historian and field archaeologist. He attempts to integrate the cultural components of bathing into the architecture of the Roman baths and to analyze that architecture typologically and historically throughout the empire.

In this ambitious and timely undertaking he largely succeeds, providing the historical and physical development of the institution from its Greek origins to its triumphant prominence in the Roman and early Byzantine world. The architectural ontology of the bath is fully revealed and so too its subsequent evolution, with special attention given to the relation between bathing practices and the particular buildings that housed them, as manifested in Rome, North Africa, and Asia Minor. Yegül's abundant illustrations and informative architectural drawings, many original to this volume, themselves constitute an impressive body of evidence; his careful analysis of the practical elements of construction, of regional variation in building techniques, and of the heating and water systems of the baths make a distinct contribution, fully grounded on a thorough knowledge of the monuments and of the recent excavations. The book contains numerous references to pertinent ancient texts and inscriptions, nicely complementing the fragmentary remnants of the rich decorations in stucco, mosaic, colored stones, and statuary that once adorned the Roman baths. What is missing, perhaps, is the intensity of the Romans' delight in spectacle as a staple of their public lives, even when mixed with water and steam.

RICHARD BRILLIANT
Columbia University

AMY RICHLIN, editor. *Pornography and Representation in Greece and Rome*. New York: Oxford University Press. 1992. Pp. xxiii, 317. Cloth \$45.00, paper \$18.95.

Since Sarah Pomeroy's *Goddesses, Whores, Wives, and Slaves* (1975), no overview of the history of women in the classical societies has been published. The subject,

however, is now enjoying such a renaissance that hundreds of articles on its different facets have appeared, many of them in single-topic volumes such as this. By now the meager evidence has been pressed to the limit and beyond, and redundancy and overinterpretation are rife. This collection edited by Amy Richlin, while not free of these defects, nevertheless shows that imaginative scholarship can still yield fresh insights. Masterminded by Richlin, the author of an excellent study of obscenity in Latin literature (*The Garden of Priapus* [1983]), it bears in part the stamp of her incisive thought and robust style. The title of the volume alludes to Susan Kappeler's *Pornography of Representation* (1986).

Able contributions by Robert F. Sutton, Jr., and H. A. Shapiro, exploring the iconography of heterosexual and homosexual courtship in Attic vase painting, largely tread ground they and others have already staked out. Sutton's observation, however, that "in the second half of the fifth century, female eroticism not only becomes respectable, but is portrayed as a means of personal happiness and social stability on vessels intended largely for feminine eyes" (p. 33), is significant enough to warrant restatement, inasmuch as the literary sources of the period give us no inkling of such a development.

Two essays deal with female figures on the Athenian stage. Nancy Sorkin Rabinowitch argues that Greek tragedy, while featuring incongruously emancipated heroines, nevertheless serves the politics of repression, a not very persuasive argument. Bella Zweig tackles a thorny problem, namely that of the strange, nonspeaking female, and nude or seminude characters that make brief appearances in Old Comedy. She concludes, rightly, I believe, that they are essentially pornographic, that is, emblematic of the degradation of women in Attic society.

Holt N. Parker uncovers the vestiges of ancient sex manuals, all of them lost, but reflected in literature, most notably in Ovid's *Ars Amatoria*, a poem that Parker considers both a sample and a parody of the genre. Poignant is his observation that ancient authors invariably attribute such manuals to female authors, a practice which Parker views as a kind of "sexual slander." In an appendix Parker provides a useful catalogue of the little-known sources for such manuals.

The weakest part of the collection are two studies, by Helen E. Elsom and Holly Montague, of gender issues in the difficult-to-document so-called Greek romances. These *erotikoi hypotheseis*, as they are called in Greek, are the first manifestation of prose fiction in the Western tradition and have come down to us in a vacuum, since we know nothing about their authors and their readership. Elsom considers them "masterpieces of literary subtlety for an educated (male) elite" (p. 212); Montague more aptly likens them to "Harlequins" (p. 231), mass-market love and adventure stories. Ignoring the most salient aspect of these novels, namely the relative independence and cour-

age of their female protagonists, these essays do little to clear up the obscurity of the social background of this literary genre. Finally, on the Greek side, Madeleine Henry, in a witty article entitled "The Edible Woman," focusing on the interminable *Sophists at Dinner* by the rhetorician Athenaeus (second and third centuries A.D.), explores the "fusion of the alimentary and the sexual" in Greek culture, yet another mechanism for the objectification of women.

On the Roman side, Molly Myerowitz's essay on pictorial pornography amply demonstrates how different the Roman sexual ethos was from that of the Athenian Greeks (and from our own). Her findings—that in Pompeii the same formulaic representations of explicit sex decorated the walls of the dingy brothels and those of the residential parts of sumptuous villas, and that in a large percentage of scenes of intercourse the woman is depicted in a position of control—are not new, but they are startling enough to warrant reformulation. Shelby Brown, in a marginally related article, directs her attention toward the gory late-Roman mosaics, mostly found in domestic settings, that depict, with sickening sadism, the suffering of man and beast in the arena, reflecting a taste for carnage in interior decoration that few would share today. An essay by Sandra R. Joshel mulls over the equation of the welfare of the state with the chastity of its women, as manifested in the myth of Lucretia (and the nearly parallel legend of Verginia), as told by Livy and other sources. She overlooks the best evidence for this phenomenon (attested for many other societies as well), namely the Roman institution of the college of Vestal Virgins.

Richlin's essay on rape stories in Ovid—more than fifty of them, some of them told in a humorous vein—unmasks the Roman poet, sometimes considered a friend of women, as a pornographer, insofar as his "gaze is male" and he is insensitive to the female experience. She uses the occasion to grapple with the dilemma that faces all students of history who are of feminist orientation. Why continue to study a historical record that is incurably male-centered and has been purged over the ages of all vestiges of accomplishments that were not?

EVA C. KEULS
University of Minnesota,
Twin Cities

SANDRA R. JOSHEL. *Work, Identity, and Legal Status at Rome: A Study of the Occupational Inscriptions*. (Oklahoma Series in Classical Culture, number 11.) Norman: University of Oklahoma Press. 1992. Pp. xvi, 239. \$27.95.

The citizen farmer, renouncing his fields to serve his country in time of war, returning only after the crisis had been surmounted, is hallowed in Roman annalistic traditions about the fledgling republic, reflecting the abiding Roman belief that agriculture was the

only respectable occupation for Roman gentlemen. From the third century B.C. on, diverse commercial opportunities arising in response to the acquisition of an overseas empire enabled Romans from every social background to acquire wealth. Although prohibited from owning seagoing vessels or competing for government contracts, members of the senatorial class covertly engaged in usury and other profitable diversions while simultaneously denigrating trades as morally contemptible. Because the literature of the late Republic and early Principate reflects this elite viewpoint, Sandra R. Joshel focuses instead on the funerary epitaphs of workers in Rome with the aim of assessing how workers themselves viewed their occupations.

Drawing on information from funerary inscriptions in volume 6 of the *Corpus Inscriptionum Latinarum* and additional supplements, Joshel has compiled a data base of 1,262 men and 208 women from Rome dating from the late first century B.C. to the late second century A.D. Excluded are senators, equestrians, imperial freedmen and slaves, actors, actresses, charioteers, gladiators, soldiers, and military support staff. Accordingly, the statistics presented deal with quite small numbers. Slaves comprise 32.3 percent of the sampling, assumed ("uncertain") slaves 13.8 percent, freedmen (former slaves) 30.8 percent, freeborn 2.7 percent, plus a further 20.4 percent of assumed ("uncertain") freeborn. Since Joshel's categorization of assumed freeborn is based on circumstantial evidence and the tenuous assumption that as some freedmen disclosed servile origins, others did not endeavor to mask it, there are probably freedmen within this group. The evidence nevertheless substantiates Susan Treggiari's contention that many shops at Rome were staffed by slaves and freedmen (*Roman Freedmen during the Late Republic* [1969], 95).

Joshel maintains that in occupational titles slaves sought dignity and identity denied to them by the depersonalized and desocialized slave experience, which disallowed family bonds and even names of their own. Likewise, freedmen were enrolled only in the urban tribes, barred from holding certain public offices, and distinguished from other citizens by formal nomenclature disclosing their former servility. The most interesting and controversial aspect of this study is the author's attempts to apply procedures devised to study the antebellum American South, as well as feminist methodologies, to re-create the personal work experience of freed and slave outsider social groups. The funerary inscriptions reveal networks of artisans joined together by their work. In large households, however, occupation served as a more precise means of identification of slaves than common names, and here Joshel's interpretation seems forced. *Colliberti* (freedmen formerly belonging to the same owner) focus on work association, not their older servile ties. Frequently the legal status of freedmen was omitted and membership or office in occupational guilds (*collegia*) was emphasized instead.

Among freedmen, such funerary epitaphs may indeed reveal reformulated self-conceptualizations ranging from reliability to idealization, illustrating both prevailing systems of occupation identification and highly individualized attitudes.

Several assumptions detract from the value of this study. When no commemorator is named in an inscription, the author presumes that the deceased composed the epitaph and that the description and sentiments expressed reflect the deceased's own beliefs rather than those of the commemorator or conventional attitudes. Joshel also supposes that the parties were conscious of the long-term sociological significance of the epitaphs. She frequently uses tomb size to note the wealth of the deceased or their families, although modest tomb size may merely reflect how much heirs or commemorators wished to spend. Joshel emphasizes the role of legal status in defining identity but minimizes the social criteria of wealth and ability that enabled many persons to achieve high social status despite the limitations of their juridical status. Finally, implicit throughout is the assumption that all workers were proud of their occupations, however menial, although few prostitutes and procurers have ever been identified as such on tombstones.

To my dissatisfaction, slightly more than one page discusses working women, yet one expects that more could be said about them. Moreover, pictorial representations on tombstones have been almost totally ignored. Readers finding the dense text and statistical tables hard going can take heart that the concluding chapter is clear, well written, and succinct.

DIANA DELIA
Texas A&M University

LEONARD A. CURCHIN. *Roman Spain: Conquest and Assimilation*. New York: Routledge. 1991. Pp. ix, 250. \$39.95.

A new, small volume on Roman Spain raises questions of purpose, but the only explanation offered by Leonard A. Curchin is the implicit intent of his subtitle: conquest and assimilation. Curchin provides a terse and readable overview of the history of the Iberian Peninsula from the beginnings of the Roman intervention to the Germanic intrusion. Successive chapters deal with the stages of conquest, Roman administration, Roman towns, and economic patterns, with two concluding chapters titled "The Romanisation of Beliefs" and "Resistance to Romanisation." The materials on the rural economy are sparse and unsatisfactory, but the section on towns provides succinct and informative summaries of contemporary information. The documentation, although light, is reasonably up to date.

Although such a book may suffice for a general audience, which seems to be the goal of the Routledge series of which it is a part, it will disappoint more

discriminating readers because it misses an opportunity to move from the conventional historiography of an earlier generation by asking new questions and exploring them with sophistication and dedication. Since Curchin has written articles on Romanization in Spain, and his final chapter is a reworked conference paper, he must be held accountable for failing to offer new insights on assimilation. One justification of historical research is that it can provide an understanding of complex processes in the past that facilitates our appreciation of equally complex issues today. Alternatively, one historical case study should ideally yield comparative insight for a comparable set of questions encountered in another time and place. Both perspectives are applicable in the case of Roman Iberia. First, conquest and assimilation underlie many current ethnic problems, which have variable time depths and demand historical as well as contemporary analysis. Can one learn from the success or failure of different Roman policies, or from the collective and individual resistance of the Iberian peoples? Second, Roman colonization and provincial assimilation, to some degree, provided models for Spanish administration of the New World; comparative study of both historical processes would be productive.

My criticism of Curchin is that his book would be of little value to someone interested in either topic, despite the promise of his subtitle. Elsewhere, challenging questions are indeed being asked about the interrelationships between Romans and barbarians, both within and beyond the frontier; an issue of *World Archaeology* (12: 1 [1980]), R. W. Brandt and J. Slofstra, eds., *Roman and Native in the Low Countries: Spheres of Interaction* (1983), and J. C. Barrett, A. P. Fitzpatrick, and L. Maccines, eds., *Barbarians and Romans in Northwest Europe* (1989) provide striking examples. Admittedly, these authors are archaeologists, but the issues they identify and the methods they use are interdisciplinary. It is true that the available data for the Iberian Peninsula are inadequate to a satisfactory pursuit of many pertinent problems. But that is no excuse for intellectual inertia in at least formulating the basic questions about what Roman conquest and assimilation of Iberia were all about.

KARL W. BUTZER
University of Texas,
Austin

YIZHAR HIRSCHFELD. *The Judean Desert Monasteries in the Byzantine Period*. New Haven: Yale University Press. 1992. Pp. xix, 305. \$45.00.

In the early Byzantine period the fifty-six-mile-long stretch of the Judean desert to the east of Jerusalem witnessed a remarkable phenomenon: the proliferation of monastic settlements as *lauras* and *coenobia*. Beginning in the fourth century, pilgrims began to

flock to the region, and many remained and became monks. Three monasteries were erected in the fourth century, and by the year 473 an additional fifteen were standing. By that year Sabas of Caesarea had begun to establish the first of ten monasteries, eight of them in the Judean desert where he had lived as a hermit as early as the year 478. Many of the new foundations sprung up along the road between Jerusalem and the Jordan River. Their staffs of monks exhibited generous hospitality, taking in pilgrims, feeding them, and caring for them in their hospices and hospitals. Toward the end of the fifth century the monastery of Choziba was converted from a *laura* to a *coenobium*, and its gates were open even to women visitors. In the sixth century most of the monasteries housed about twenty monks—one or two had perhaps some 400 monks—and a total of some 3,000 monks are believed to have occupied the region in its heyday. Only after the Persian invasion of 614 and the Muslim conquest of 638 did Judean monasticism decline precipitously, although a few monasteries along the road from Jerusalem to Jericho kept their doors open to visitors.

Since 1967 the number of recorded early Byzantine monastic sites in the Judean desert has more than doubled. For a decade, beginning in 1981, Yizhar Hirschfeld systematically examined the fifty sites that had been previously known and proceeded to discover another dozen himself, so that some sixty-two were known by 1990. Three sites he actually excavated. In this book he offers the first comprehensive study of this monastic architecture. It is a welcome addition to early Byzantine studies. He describes and interprets the results of archaeology of these various Judean desert sites and pertinent hagiographical literature, known to him from Hebrew translations from original Greek sources. Presented in nine chapters, his material is readable, thoroughly documented, and includes an extensive up-to-date bibliography. An appendix offers a short biographical sketch of nineteen major figures in the history of early Byzantine Judean monasticism. Seven maps, 130 black-and-white figures, seven tables, and a useful index accompany the volume.

Hirschfeld discusses how the monasteries were laid out and built, their workmen and patrons, the religious and secular components of their architecture, the daily life of their inhabitants, their sources of livelihood, martyrial sites in the desert, and hermits and their way of life. He presents a clear typological analysis of the layouts of these monastic sites. They bear resemblance to monasteries at other localities, such as Alahan in southern Anatolia of the late fifth century and the sixth-century monastery of St. Catherine at Mount Sinai. This resemblance is explained in part by the fact that the monks settling in the Judean desert originated outside of Palestine. With one exception, however, the church buildings of these monasteries were small and simple single-naved structures terminating in an apse, in contrast to more elaborate

monastic planning elsewhere. Architectural sculpture characterizing the monasteries at Alahan and Mount Sinai is lacking in the Judean establishments, but its absence certainly does not demonstrate monastic poverty. Rather it reflects local tradition recorded in the region during Roman imperial times. Although a few of the Judean monasteries were designed and constructed by professional masons and even featured mosaic pavements, stuccoed walls, and glass window panes, the majority of them must have been planned by the monks themselves who used local materials.

Hirschfeld enlivens these ruins of monasteries by reconstructing from archaeology and written sources what is known about their inhabitants. He discusses the simple bread and vegetable diets of the monks, their production of oil and wine, their dress, sleeping arrangements, burial customs, and other evidence of their material culture. A striking feature of these sites was their water systems, which featured cisterns, canals, and ceramic gutters. The resourcefulness of these early Byzantine monks calls to mind that of modern-day Israelis.

W. EUGENE KLEINBAUER
Indiana University,
Bloomington

MEDIEVAL

ARNO BORST. *Medieval Worlds: Barbarians, Heretics and Artists in the Middle Ages*. Translated by ERIC HANSEN. Chicago: University of Chicago Press. 1992. Pp. xiii, 273. \$39.95.

This book collects articles on a wide variety of topics written over the course of a long and productive scholarly career. It is a tribute to the range of Arno Borst's interests that the essays making up this collection are hard to summarize: the subtitle offers three categories, but the book itself is divided into eight parts and even then the links between one essay and another sometimes seem arbitrary. The essays are mixed in character as well as subject, some originally written for a general audience and others for specialists; a few, apparently, have not been published before.

The popular or public aspect of some chapters is especially appropriate because Borst himself, as he explains in his introduction, sees history as "man's attempt, in an always confusing present, to form his social existence in a way that will let it withstand the constantly threatening future, and also to come to terms with a repeatedly insurmountable past" (p. ix). Indeed, certain themes do emerge despite the diversity of subjects discussed. One is the effort of people to cope, with nature and with each other. Another is the effort of the modern historian to understand the past, less as a guide to present choices than as a mirror in which modern options take their place in a broader range of human experience.

Borst's sense of history as an enterprise based on

reflection as well as science finds expression, too, in many of the individual essays, particularly the opening chapters on the use of the term "barbarian" in European history and on the concept of language from Isidore of Seville to Joseph Stalin. Of similar spirit is the concluding essay, which perhaps deserves special mention for the perspective it offers on what the last sixty years of European history have meant for Germans of Borst's generation. It is, in fact, a brief personal memoir in which Borst casts an eye back over his own life: Borst notes his boyhood experiences in Lower Franconia, his abrupt education when he was called to war in 1943, his professional formation under Hans Heinrich Schaeder, Percy Ernst Schramm, and Herbert Grundmann, and the evolution of his scholarly interests from heresy to language to regional history and finally back to religious and cultural history.

Other essays, while generally broadly focused and intended to offer reflections on historical problems, reveal a broad and thoughtful acquaintance with the sources of medieval history. Such is the character, for example, of the essays on world history in the Middle Ages, on medieval heresies as mass movements, saints as city patrons, and on science and games (this last an ingenious effort that juxtaposes a computer game called "Dueling Digits" with the medieval *Rithmimachia*, in which players attempted to create numerical harmonies instead of accumulating points calibrated as profits). His "Three Studies on Death in the Middle Ages" juxtapose the dying experiences of Wetti of Reichenau (d. 820)—who himself had written of the death of St. Gall—two cases from twelfth-century Holstein, and Giovanni Boccaccio. An essay on women and art ranges from Roswitha to Alessandra Macinighi, a widow in fifteenth-century Florence. Additional topics discussed include the medieval conception of public persona, crisis and reform in fourteenth-century universities, and the social context of the fifteenth-century witch persecutions of the Alpine Simme Valley.

Even the more narrowly focused essays can open out suddenly. Perhaps the best example is Borst's discussion of Abelard's conception of time, in which he correctly juxtaposes Abelard's theoretical comments in his *Dialectica* with the sense of lived time of the *Historia Calamitatum*, only to bring in unexpectedly at the end not only Jacques Le Goff's notions of church time and merchant time but also the historian's time: time as *scientia* rather than *experimentum*.

The book as a whole will perhaps be of interest mainly to an educated general public. Specialists might prefer to dip in here and there rather than to read the book straight through, but the balance of judgment and observation they find here may lead them to investigate some of Borst's other work.

CHARLES M. RADDING
Michigan State University

HEINRICH FICHTEAU. *Ketzer und Professoren: Häresie und Vernunftglaube im Hochmittelalter*. Munich: C. H. Beck. 1992. Pp. 351. DM 98.

Heinrich Fichtenau, the dean of Austrian historians of medieval Europe, has long been one of the few scholars possessing both extraordinary technical skills in paleography and diplomacy and the ability and imagination to lead from these fields into the history of culture and ideology. He is the pioneer of modern studies of medieval literacy and of the ideology and world-views of those who created imperial charters. He is best known as a historian of disorder in societies otherwise thought to be orderly (*The Carolingian Empire* [1949; 1957]) and as a historian of elements of order in societies long considered highly disorderly (*Living in the Tenth Century: Mentalities and Social Orders* [1984; 1991]).

The present book deals with a period later than the two cited above, and with a topic more narrowly focused: the rage for authentic knowledge on the part of both religious dissenters and scholars in the eleventh and twelfth centuries and the response of authority to both. In this century, scholars have usually considered these two phenomena separately. But lately scholars have produced expanded surveys of the field. Both Jeffrey Burton Russell's *Dissent and Order in the Middle Ages* (1992) and the extensively revised edition of Malcolm Lambert's *Medieval Heresy* (1977; 1992) have treated these subjects. Thus, Fichtenau's study is an original and major contribution to a field recently reshaped.

The first section of the book ("The Heresies") treats in considerable detail the emergence of devotional dissent, the scholarly tradition of explanation, and the religious-political background, concluding with a critical analysis of the romantic and egalitarian interpretive myths of the nineteenth and twentieth centuries. The third section ("The Empire of Reason") outlines the debate between reason and authority, describes the academic disciplines in twelfth-century schools, the structure and social importance of the new schools, and the character of the attacks on Peter Abelard, Gilbert of Poitiers, and others who charged the scholars with heresy.

The core of the book, and the link between the heretics and the scholars, is the second section, "Myth and Mystery," an analysis of the governing myths of Bogomils, Cathars, and Platonists, the allegorical-literary transformation in twelfth-century biblical exegesis, and the consequences for later centuries of the new rationalism of the schools.

For Fichtenau, what characterized both dissenters and scholars was their insistence on reason as the criterion of truth. For a time, the scholars succeeded better than the heretics, whose insistence on reason was not as complete and who encountered doctrines head-on that scholars could avoid challenging or could challenge in more subtle and acceptable ways. Fichtenau concludes: "It is here that the borderline

appears to run, not between orthodoxy and heresy, but between intellectual and popular religion" (p. 286). Once again, Fichtenau has rethought conventional characterizations of order and disorder and once again redefined them in a study that is a major contribution to scholarship in the eleventh and twelfth centuries.

EDWARD PETERS

University of Pennsylvania

AARON GUREVICH. *Historical Anthropology of the Middle Ages*. Edited by JANA HOWLETT. Chicago: University of Chicago Press. 1992. Pp. xii, 247. \$39.95.

This collection brings together a number of essays from the 1970s and 1980s by the Russian historian Aaron Gurevich. Divided into two groups, "Methodology" and "Case Studies," they either tend to be a critique of other historians, chiefly the *Annalists*, or to focus on the literature of medieval Scandinavia, the area with which Gurevich's earliest work known in the West was concerned. Virtually all the essays were published previously in either English or French; only two appear here for the first time translated from Russian. These are comparatively slight pieces: an essay dating from 1979 on the humorous Old Icelandic saga of "Thorstein Goose-Pimples" and a talk given in 1987 in which Gurevich outlines the essentials of his own methodology and his understanding of the concept of historical *mentalité*.

Although these essays are neither new nor unknown to Western historians, a number of them have not been easily available, and in any event it is worthwhile to have in one volume the essays that made Gurevich's longer work anticipated among cultural historians, culminating with his best-known book, *Medieval Popular Culture* (1988). From *Annales* are reprinted here both Gurevich's 1982 critique of Philippe Ariès, "Au Moyen Age: Conscience individuelle et l'image de l'au-delà" (translated into English for this volume), and his 1983 essay criticizing Jacques Le Goff and other *Annalists* for their ingenious disavowals of ideology in their work (a claim prompting from Gurevich the wry observation, "It is hard to imagine a scholar without a theory, even if only an implicit one" [p. 33]). Literary historians in particular will be glad to see the essay (1984) on "Oral and Written Culture of the Middle Ages: Two 'Peasant Visions'" (on the *Vision of Thurkill* and the *Vision of Godeschalk*) and the long 1982 essay "On Heroes, Things, Gods and Laughter in Germanic Poetry." Besides these classics are a number of shorter pieces on Scandinavian culture, less well known except among specialists in that field, but all carrying the distinctive Gurevich stamp, a preoccupation with "popular" agrarian, kinship-based village cultures, particularly of northwestern medieval Europe, focused through a distinctively Marxist-materialist lens.

This present collection is not, I think, the best

introduction to Gurevich's thought, since his methods are on more complete display in the book-length studies already published in English. Yet there is much material here from which even historians who have thoroughly digested those books will profit. A succinct analysis of the west-portal tympanum at Autun lays out rather better than some of his more familiar work Gurevich's conviction that twelfth-century *mentalité* encompassed a contradictory, temporally indeterminate version of the Last Judgment, and hence of human history: one ecclesiastical and elite, the other personal and popular. The three short essays that close this volume, all bearing on the conception of "private property" among Scandinavian peoples, should stimulate historians to consider this comparatively neglected area of medieval culture in analyzing ideas of community and subjectivity that have been more commonly discussed in reference to France or Italy.

In a brief preface Gurevich describes his intellectual development. Although his work, which paralleled the conclusions of the Western *Annalists* closely yet appeared to have been conceived in isolation, took many Western historians by surprise a decade ago, Gurevich is at pains to make clear that he thinks of himself not as a solitary genius but as one of a community of Soviet historians; as he puts it, "we also have our own traditions to build upon" (p. xii). The coherence of these traditions is apparent throughout this volume.

The differences from Western traditions are also apparent. This is not simply a matter of subject matter or emphasis. Most striking, to my mind, is what Gurevich makes of religion, for he seems to assume that Christianity was always and everywhere an elite dogma imposed uneasily on an inchoate, peasant, pagan "belief." He also seems to believe that religion of any kind is best addressed as psychology. American anthropologists have developed a more rhetorically based analysis of religion in culture than this; it will be interesting to see how Russian historiography responds, now that the barriers to exchange are down.

MARY J. CARRUTHERS
New York University

STEVEN F. KRUGER. *Dreaming in the Middle Ages*. (Cambridge Studies in Medieval Literature, number 14.) New York: Cambridge University Press. 1992. Pp. xii, 254. \$59.95.

As the title of the series in which this book was published indicates, its target audience is literary scholars. With respect to them, Steven F. Kruger argues that the dream-vision, as a medieval literary genre or *topos*, is less one-dimensional than others have claimed. First, it drew on philosophical, scientific, and theological sources and not just on literary ones. And second, dreams and visions served a range

of literary functions besides educating the literary character or author portrayed as experiencing them. He develops this thesis in the last two chapters of his book. In the first five chapters he seeks to trace the intellectual history of dreams and visions up through the later Middle Ages, so as to contextualize the literary works he discusses in his final chapters. The first part of the book is thus of considerable potential interest to historians. But it has to be reported that both parts of the volume under review go only part of the distance toward Kruger's intended destination.

In the first five chapters, his problem is partly organizational and partly a function of his selectivity. Instead of beginning at the beginning, with the biblical and classical sources of medieval views on dreaming and visionary experience, Kruger starts in *medias res*, with two chapters on high medieval dream-books and the reactions of canonists to them, before backpedaling to take up the church fathers and transmitters who open the Middle Ages. Although he is aware of the fact that other philosophical schools, no less than the Greco-Roman religious culture, held positions on dreams and visions, what he wants to accent here are two points: first, the notion that the patristic authors adopted a Neoplatonic view of dreams and visions, which they regarded as basically spiritual experiences, although there was some acknowledgment of the idea that they could have somatic causes, and, second, the notion that they could be the effects of superhuman beings, good or evil, on the subject as well as having an internal etiology. The next two chapters chart the influence of this patristic model through the thirteenth century and the heavy competition it encountered, with the reception of Aristotle and Greco-Arabic science, from the side of medicine and natural philosophy, whose proponents pushed for a more somatic understanding of the etiology of dreams and visions. Both of these positions, Kruger concludes, remained options in the High and later Middle Ages, for thinkers as well as for writers.

In developing this intellectual history of dreams and visions, Kruger's coverage is alarmingly skewed. He omits some very obvious sources, such as saints' lives, exegeses of the books of the Bible in which dreams and visionary experiences are reported, and most of the mystical tradition of the High and late Middle Ages. With respect to the latter, Hildegard of Bingen is mentioned as a naturalist, Richard of St. Victor as a commentator on Joel, but neither is seen as a mystic. The Cistercian, Franciscan, and Dominican schools of mysticism, as well as the Eucharistic or liturgical mystics, are virtually ignored. The best documented visionary of the Middle Ages, Joan of Arc, is not mentioned. Although Julian of Norwich and Margery Kemp are treated, they are presented as writers, not as thinkers. Kruger notes the tensions between the Neoplatonic and Aristotelian-medical theories among the scholastics, observing that even those who, like Albert the Great, favored the Neopla-

tonic theory, were constrained to express their views in the language of Aristotelian epistemology and faculty psychology, but he ignores the post-Aristotelian epistemologies of John Duns Scotus and William of Ockham, and the host of later scholastics whom they influenced. The only fourteenth-century thinkers whose positions on dreams and visions he discusses are Nicholas Oresme and Giovanni Boccaccio, the latter confusingly presented as a thinker and not a writer.

When it comes to the literary examples proper, we also find that Kruger has made only a part of his case. Aside from ignoring pertinent scholarship (Tullio Gregory, ed., *I sogni nel medioevo: Seminario internazionale* [1983]; Agostino Paravicini Bagliani and Giorgio Stabile, ed., *Traume im Mittelalter: Ikonologische Studien* [1989]. The first collection deals with texts, the second with pictorial art, an area of nonliterary source material nowhere acknowledged by Kruger), the texts he presents raise some question about his principles of selection. Certainly, the cases cited support his claim that literary dreams and visions have more complex functions than those assigned to them by some scholars. One is struck, however, by some remarkable omissions. These include the early Irish poems in which dreams transport characters to the other world and back again, the Old English *Dream of the Rood*, Kriemhild's premonitory dreams in the *Nibelungenlied*, and, most astonishingly, the *Divine Comedy*. The fact that the *Romance of the Rose* is described, at its end, as having been a dream-vision of the lover throughout, eludes Kruger's notice. Equally important, since he basically ignores fourteenth-century scholasticism, especially as it would have been available in England, his effort to argue that it is contemporary nonliterary thought that provides the context for interpreting authors like William Langland and Geoffrey Chaucer fails to persuade.

In sum, while some interesting and valuable material is brought out in this book, there is a great deal more to the story of what dreams and visions meant to medieval thinkers and writers than we find here. Kruger can be regarded as one among several scholars who have sought to open up this subject. But his book cannot be seen as having covered it, or even as having mapped it, with the thoroughness it deserves.

MARCIA L. COLISH
Oberlin College

V. KRAVARI, J. LEFORT, and C. MORRISON, editors. *Hommes et richesses dans l'Empire byzantin*. Volume 2, VIII^e-XV^e siècle. (Réalités byzantines, number 3.) Paris: P. Lethielleux. 1991. Pp. 390; 12 plates.

This is an important publication, treating various aspects of the Byzantine economy and society. It includes eighteen articles, and comments on three of the articles. Some of the studies are synthetic, while others present original research. Together they constitute a notable contribution to Byzantine studies.

A number of themes run through the volume. First, the editors (and many of the authors) place Byzantium firmly within a wider European or, perhaps, Mediterranean perspective. This is achieved either through explicit comparison (as, for example, in the article by J.-M. Martin and G. Noyé on the cities of Byzantine Italy), or through the adoption of the methodological apparatus used in the admittedly more developed field of Western medieval studies, or through an effort to identify concurrent developments in Western Europe and Byzantium. Comparisons with Islamic societies are rarely drawn.

Second, it is argued that the Byzantine empire witnessed a virtually continuous economic development from at least the tenth century through the first half of the fourteenth, with the eleventh and twelfth centuries constituting something of a take-off. The rehabilitation of the eleventh and twelfth centuries, which thirty years ago were considered a period of decline, is not surprising, being, rather, part of an emerging new orthodoxy, which is here supported by new evidence and argumentation. More novel and more surprising is the idea that healthy economic and demographic trends continued in the thirteenth century and the first half of the fourteenth century, despite profound political troubles and the establishment of an Italian-dominated system of exchange in Byzantine territory and elsewhere. This concept of virtually uninterrupted expansion is challenged occasionally here: N. Oikonomides, in a valuable article, raises the possibility (but without developing it) of a profound demographic crisis in the early eleventh century (pp. 336–37); John Day, in his commentary on Cécile Morrisson's study of money and finances, wonders whether the secular growth of the Byzantine economy stopped a hundred years earlier than that of Western Europe, that is, in the early or mid-thirteenth century (p. 319).

This brings us to a third recurring suggestion of the volume, namely that the development of the Byzantine economy in the period under consideration has the same traits and the same chronology as that of Western Europe. The suggestion is made most unambiguously by J. Lefort and J.-M. Martin (see also p. 82). It is brought into question implicitly by Oikonomides, who sees a differential development in agriculture (p. 336). The problem is a difficult one, not least because the economic development of Western Europe was not uniform.

Because it is not possible to discuss all of the studies included in the volume, I will comment on only a few. G. Dagron's work on burials in Constantinople is a fine example of a new approach to the study of Byzantine society. The topic gives the author the opportunity to present a masterful examination of urban institutions, including corporations and confraternities. He makes incisive comments about the fiscal practices of the Byzantine state and about the regulatory and legislative role of the state, to conclude that the image of complete state control is an

illusion, fostered by the Byzantine legislators themselves, and that Byzantine legislation, rather than shaping the evolution of society, simply confirmed it. The most significant contribution of the essay is the recognition of the importance of associative institutions in Byzantium. Equally instructive is the author's subtle comparative approach: the specificities of Byzantine society are established, and the assertion of the importance of corporative institutions invites, without forcing it, comparison with other medieval societies. What emerges is a much more interesting and complex picture of Byzantium than the somewhat reductionist traditional image of an authoritarian society with powerless citizens.

Morrisson has contributed a study of money and finances that combines monetary and economic history and successfully places Byzantium within the broader spectrum of monetary movements in medieval Europe. She also draws on knowledge of modern economic systems and on the science of economics to establish probabilities where certainties are lacking. Her study of coinage supports the idea of an economic expansion from the tenth through the twelfth century, with a decline starting in the thirteenth century. The essay is followed by an exemplary commentary by Day.

Because the Byzantine economy, as that of most preindustrial societies, was based on agriculture, the study of the rural economy and society is of fundamental importance. J. Lefort, who has already made major contributions to the study of Byzantine agrarian history, has three articles in this volume, one a synthetic study coauthored with J.-M. Martin. In earlier work on the development of various habitations in Macedonia, he documented the expansions of cultivated and inhabited space from the tenth century onward. It is primarily through the study of the landscape that he posits here a demographic expansion in the course of the tenth through the twelfth centuries, since other documentation on population is sparse for this period. Of particular interest is the discussion of the multiplication of domains, which, created by laymen and ecclesiastics, are a first indicator of economic expansion in the countryside. Lefort finds a doubling of agglomerations between the tenth and the first half of the fourteenth century (p. 72). The question is, when did this expansion slow down or end, and why? According to Lefort, there is an uninterrupted demographic expansion until the middle of the fourteenth century; the main reason for the subsequent decline was the Black Death. He thus finds a clear and complete coincidence of expansion and decline between Byzantium and Western Europe (or, perhaps, with the Mediterranean countries) and an external, catastrophic cause for demographic decline. I cannot entirely agree with this.

I have argued elsewhere that the population of Macedonia was already declining in the first half of the fourteenth century. The considerable data from this period (which Lefort has also studied intensively)

show a progressive decline and aging of the peasant population, as well as a decrease in the size of the household and an impoverishment of the population (see my *Peasant Society in the Late Byzantine Empire* [1977], chaps. 6–7). Lefort accepts an economic crisis but not a demographic one (pp. 75 and following). I remain convinced that the population figures show that the two crises were concomitant.

As for the cause of the eventual decline of population, the effects of the plague remain unknown, since the sparse Byzantine sources provide no details and do not allow us to distinguish between its effects on town and country, poor and rich. The negative effects of endemic warfare on the demography of the countryside are accepted by Lefort for the tenth century (the period of the Bulgarian wars, p. 68, and see p. 81 on the Ottoman peace), but are not given much weight for the fourteenth century; yet the sources speak of the flight of peasants because of the civil wars of the first half of the fourteenth century and this, along with impoverishment and dwindling resources, should be taken into account.

Although one may not agree with every detail in every article of this volume, and although some of the conclusions also may be arguable, there is no question that this is a valuable contribution to the study of Byzantium. It is also an important book for students of medieval societies generally. We are becoming increasingly aware of the fact that these societies shared important characteristics. To my mind, it is not necessary to establish exact chronological parallels, which is, in any case, impossible to do given the diverse development of Western Europe and indeed of the constituent parts of the Byzantine empire. What is important is to look at each medieval society with an informed eye that can discern both the similarities and the differences between the object of study and other parts of the medieval world; in the case at hand, Byzantium certainly shared traits of Western medieval societies, and we are still discovering how many; but it also had a number of fundamental differences. The reader will find, in this volume, guidance to both.

ANGELIKI E. LAIOU
Dumbarton Oaks
Harvard University

DONALD M. NICOL. *The Immortal Emperor: The Life and Legend of Constantine Palaiologos, Last Emperor of the Romans*. New York: Cambridge University Press. 1992. Pp. xiii, 148. \$39.95.

Donald M. Nicol has written a moving and frankly sympathetic biography of the last Byzantine emperor, Constantine XI Palaeologus (1448–53). This now becomes the standard biography, replacing the century-old amateur but consciously political work by the Serbian diplomat Constantine Mijatovich (*Constantine, the Last Emperor of the Greeks* [1892]). Nicol once

described his own style to me as “Runcimanesque,” and he is, after all, a rare student of Steven Runciman. But this book is not a mere rehash of Runciman’s account of the fall of Constantinople (*The Fall of Constantinople 1453* [1965]) or of the author’s own chapter on Constantine XI in his *The Last Centuries of Byzantium* (1972). It is a delightfully written, solidly researched, and appropriately detailed biography that nevertheless reads easily.

Although insightful, however, it is a narrative, without attention to social, economic, or institutional history. The difficult issue of the reunion of Orthodox and Roman Catholic churches receives judicious attention. Nicol envisions Constantine XI as both a tragic and heroic figure. He carefully probes accounts of the death of Constantine XI on May 29, 1453, during the Ottoman storming of Constantinople. A major contribution to the history of the Palaeologan dynasty, it includes an investigation of the metahistory of Constantine XI, especially such curiosities as later pretenders to the Palaeologan succession down to the twentieth century.

Nicol tries to sort out fact and legend. His judgments are generally prudent, yet some aspects of legend could receive more amplification. The various predictions and Turkish supernatural fears about their own possible doom, which caused the sealing of the famous Golden Gate at Constantinople (pp. 102–04, 109–10), should be compared with legends of the sealed Golden Gate at Jerusalem (here would be another fascinating book); and a more comprehensive review of hostile foreign predictions about the eventual end of Turkish rule of Constantinople should include those investigated by Kenneth M. Setton in *Western Hostility to Islam and Prophecies of Turkish Doom* (1992). The Greek titles also needed more careful proofreading, especially to correct errors in the diacritics (irrespective of the fluctuating state of demotic Greek orthography).

This book is attractively printed, with well-chosen illustrations, a good bibliography, and an index. The book is highly recommended, both for a broader educated readership as well as for more specialized Byzantinists, church historians, Neohellenists, Ottomanists, Balkanists, medievalists, antiquarians, and genealogists. Let us hope that the author will similarly tackle the reign of John VIII.

WALTER EMIL KAEGI
University of Chicago

JAMES M. POWELL. *Albertanus of Brescia: The Pursuit of Happiness in the Early Thirteenth Century*. (Middle Ages Series.) Philadelphia: University of Pennsylvania Press. 1992. Pp. xii, 147. \$22.95.

Viewed from north of the Alps, the figure of Albertanus of Brescia is an anomaly. A lawyer, judge, and unsuccessful soldier, he composed from 1238 to about 1250 three treatises on human happiness and

the ordering of society, subjects usually treated by theologians. A father, he wrote in Latin, the language of the celibate clergy, and dedicated his treatises to three sons, who presumably could also read them. A layman, he delivered at least five sermons in a church, a skill that had become the specialty of the mendicant preaching orders. His works, surviving in hundreds of manuscripts and soon to be translated into Italian and French, were read by Brunetto Latini, Dante, Gower, and Chaucer and thereby brought Albertanus to the attention of modern scholars as a precursor to the humanists.

Now James M. Powell wishes to expound the intellectual concerns of Albertanus in his own context of urban Italy in the thirteenth century. After situating Albertanus in the politics and society of thirteenth-century Brescia, he devotes a chapter to each of the treatises and to the sermons in which he contextualizes the works by identifying the sources and the historical concerns to which each was addressed. The *De Amore et delectatione Dei et proximi et aliarum rerum et de forma vitae* (1238), which drew on the moralist traditions of Seneca and the Old Testament wisdom literature, articulated the social aspirations of the communal society and proposed the moral reform of the individual. The *De Doctrina dicendi et tacendi* (1245) marshaled the rhetorical traditions of Cicero and Alcuin to discipline the power of language in service of the legal profession. In the form of a dialogue between a husband and a wife, the *Liber consolationis et consilii* (1246) employed these sources along with Publilius Syrus to propose remedies to the vendettas endemic to the urban society of the Italian commune. His five sermons preached at the church of San Giorgio Martire (ca. 1250) offered spiritual and moral advice to the confraternity of his fellow lawyers and judges. Throughout these chapters, Powell stresses the underlying urban violence of thirteenth-century Brescia, where the pope fought with the emperor, Guelfs with Ghibellines, the bishop with the abbot, the orthodox with the heretics, and the aristocracy with the craft guilds. By uncovering the sources and sketching the broad contours, he exposes the generalized and moralistic solutions of this literate layman to the ills of his times.

Although Powell's major theme of Albertanus's response to urban violence is well developed, his slim volume raises other questions that pique one's curiosity. Since Albertanus's principal treatise, *De Amore*, is of difficult access (Powell uses an unpublished edition by Sharon Hitz), one would like to know more of its contents and salient lines of argument. Albertanus is the first known author to have read and quoted extensively from Andreas Capellanus's *De Amore*. Powell recognizes this fact in a single sentence: "Albertanus . . . used Andreas chiefly to provide discussion of particular kinds of human relationships and most often disapproved of his views" (p. 46). Disapproval or not, a sustained analysis of Albertanus's employment of Andreas's courtly text would

further illuminate the former's views on love in his own *De Amore*. (See the preliminary remarks of Alfred Karnein, *De Amore in volkssprachlicher Literatur* [1985], 110–14.) Although Powell cannot find contemporary precedents for Albertanus's rhetoric, a more penetrating analysis of the rhetorical praxis in the *De Doctrina* would certainly help to situate him in the doctrinal traditions of the discipline. And, finally, Albertanus's platitudinous style undoubtedly obscured much of his social context. Powell complains that "the real world of concrete experience and specific institutions finds little place in his writings" (p. 11). Yet to take just one example, Albertanus's remarkable rejection of the sumptuary legislation against women cries for elucidation from all that we have learned about late-medieval sumptuary practices. Powell's study opens to our inspection the fascinating figure of an Italian literate layman in the thirteenth century and stimulates our appetite for a closer look.

JOHN W. BALDWIN
Johns Hopkins University

GERHARD RÖSCH. *Der venezianische Adel bis zur Schliessung des Grossen Rats: Zur Genese einer Führungsschicht*. (Kieler Historische Studien, number 33.) Sigmaringen: Thorbecke. 1989. Pp. 279. DM 54.

With this book, Gerhard Rösch becomes the foremost contemporary interpreter of medieval Venice. He has critically reexamined the sources of Venice from its founding to 1300 in this and in his earlier volume, *Venedig und das Reich* (1982). The present study defines the ruling elite of Venice. Rösch's method is to identify the members of the Venetian ruling aristocracy and to draw conclusions from this identity. This may seem easy, but he has had to pull aside the myths of family history and tradition that developed in the Renaissance.

By searching in the medieval documentary sources and not accepting tradition, he has collected names of the Venetian nobility up to the "Serrata" of the Great Council in 1297. From the city's founding to the eleventh century, when individuals can scarcely be identified, the author neatly summarizes Venetian political and social history. Surnames first appeared in ninth-century Venice on the Byzantine example, but the earliest lists of Venetian leaders appeared in the chronicle "Origo civitatum Italie seu Venetiarum" (R. Cessi, ed., *Fonti per la storia d'Italia*, 73 [1933], the so-called "Chronicon Altinate"). The "Origo" listed the Venetian ruling elite twice, the first list dated not later than the middle of the tenth century and the second list dated the beginning of the eleventh century. In addition to these "Origo" lists, Venetian patricians signed as witnesses to the surviving acts of the doges. These names were not precisely identical with those twenty-four families that Renaissance chroniclers termed the "Founding" or "Apostolic

Families." Some politically active families had died out before the Renaissance, some were added. Rösch describes the social layers in medieval Venice, their wealth based on commerce as well as landholding, and their social mobility. He emphatically denies that feudalism, as a social, military, or political system, took hold in Venice. For Venetians, warfare meant sea battles, not cavalry sorties. Landholding on the few acres of the Venetian lagoons owed much to late Roman practice and nothing to feudal land tenure.

The Venetian Commune after 1141 was led by elected doges and served by appointed or elected magistrates. The number of officeholders increased greatly after the Fourth Crusade's expansion of Venetian interests throughout the eastern Mediterranean, because the city assumed control over its overseas possessions and subjected its own economy to tighter regulation. Rösch declines to repeat earlier authors' analyses of the diminishing power of the doge under the commune. Rather, he creates lists of officeholders. These he divides into five groups, weighted according to the degree of their participation in political affairs. He concludes that the principal old families, with few additions, continued to dominate Venetian politics up to 1300. Rösch disputes the thesis of Marxist class warfare in Venice. Because the upper layer of society was continually enriched from lesser mercantile families, because opportunity for advancement beckoned all ambitious Venetians to their overseas dominions, because the "citizen" class solely was permitted to provide notaries and grand chancellors to Venice, and because craft guildsmen were permitted limited self-government and civic responsibility by the ruling elite, they succeeded in keeping a relatively peaceful city during the thirteenth century. Guelf-Ghibelline civil strife did not occur in Venice.

The climax of the thirteenth-century social development was the so-called "Serrata" of the Great Council in 1297. Rösch accepts completely the interpretation of Frederic C. Lane ("The Enlargement of the Great Council," *Florilegium Historiale: Essays Presented to Wallace K. Ferguson* [1971], 236–74). This event of 1297 increased the number of Venetians eligible for the Great Council, and thus for election to the multitude of public offices.

Relations between the Venetian patriciate and the church form an adjunct chapter. The church lost any influence in Venetian political affairs under the commune. Nevertheless, the author names the patriarchs of Grado, the bishops, and the more important abbots and abbesses to demonstrate the near monopoly by Venice's nobility on their own high ecclesiastical offices. Another book could, in the future, possibly develop his suggestion that the mendicant orders became more powerful in Venice after 1260.

The strengths of this book are its identification of the ruling families of Venice and its reconsideration of Venetian constitutional changes up to 1300. The

book closes with lists of officeholders, bibliographies, and indexes of places and persons.

LOUISE BUENGER ROBERT
University of Missouri,
St. Louis

GIOVANNA PETTI BALBI. *Una città e il suo mare: Genova nel Medioevo*. (Biblioteca di storia urbana medievale, number 5.) Bologna: CLUEB. 1992. Pp. 362. L. 42,000.

Giovanna Petti Balbi's essays on the history of medieval and Renaissance Genoa follow a common format. Balbi knows the sources of Genoese history as well as any historian in the world, and when she considers a problem she marshals a considerable body of evidence to illustrate her theme. For example, the essay on images of Genoa gathers impressions of travelers from St. Bernard to Petrarch to Francesco Filefo in order to compensate for the lack of Genoese descriptions of their own city. Sometimes this method produces a catalog of data, as when Balbi collects all the names of pre-1250 ships, revealing over time a slight shift in favor of saints' names over profane ones. Supplying this information is no substitute for analysis, however, and there is no explanation here for the change. The study of work and apprenticeship contracts collects all surviving contracts from 1257, hence the comprehensive picture of urban work is detailed yet static.

In most of these essays, Balbi supplies fresh and creative answers to some old problems. Balbi's articles on Genoese expansion in the Mediterranean look closely at the city's colonies, always a major interest of Genoese historiography. Local or national patriotism supplied reasons for studying the colonies in the Black and Mediterranean seas, but, as Balbi points out, when Genoese history is placed in this broader context the problems of medieval colonialism become more general and important. Wherever the Genoese went, they made another Genoa; so observed an anonymous poet of the early fourteenth century. The *madrepatria*, to use the evocative Italian word for homeland, tended to place colonies like Chios and its share of Cyprus in the hands of the creditors of the state, letting them rule, collect taxes, and bear the expenses of government and defense. Genoa's exotic Mediterranean empire was really a collection of privatized concerns, and Balbi's studies of Caffa and Cyprus are illuminating and important. Corsica, sometimes ruled by the city government and from 1378 to 1407 in the hands of a private company, was the most durable Genoese experiment in colonization.

As the editor of the fifteenth-century historians Giorgio Stella and Giovanni Stella, Balbi is well known for her insightful work on city chroniclers, from Caffaro in the twelfth century to the humanists of the fifteenth. A distinctive feature of Genoese

historiography is that from the beginning it was in the hands of lay people, either important nobles like Caffaro or Jacopo Doria, or the notaries appointed to keep an official record of the commune's activities. But local culture also produced important writers like Jacopo da Varagine, author of the *Golden Legend*, and Giovanni Balbi, compiler of one of the great medieval Latin dictionaries, as well as poets, mathematicians, herbalists, and physicians. As Balbi observes, Genoa did not have a university or a princely court, so its culture tended to be dominated by what merchants would read or pay for, hence the practical cast to much of its literature.

In the introduction Balbi stresses that the most important influence on Genoese history was the sea; this city was, and is, the quintessential port town. The mountains prevented Genoa from expanding significantly except along the coast. So the Genoese went "down to the sea in ships that do business in the great waters," to use the words of the psalmist, always meaningful ones in Genoa and appearing in Balbi's epigraph. So much of Genoese history took place far from home, wherever adventurous Genoese served their own state or someone else's. Balbi's exemplary work, balanced as it is between studying the city itself and its people in the wider world, reveals the ways in which a talented historian can use Genoa's marvelous sources.

STEVEN EPSTEIN
University of Colorado,
Boulder

PENELOPE D. JOHNSON. *Equal in Monastic Profession: Religious Women in Medieval France*. (Women in Culture and Society.) Chicago: University of Chicago Press. 1991. Pp. xv, 294.

Penelope D. Johnson's informative study of northern French nuns in the eleventh, twelfth, and thirteenth centuries draws primarily on twenty-seven "documents of practice": cartularies from twenty-six female religious houses and the register of Archbishop Eudes of Rouen (1248–69), who provided lively discussions of his numerous visits to eighty-two religious houses (sixty-one male, fourteen female, and seven mixed).

Johnson argues that until the mid-twelfth century, the activities and social/religious functions of nuns closely paralleled those of monks. Like monks, nuns received financial support from lay benefactors, who greatly desired their intercessory prayers. And like their male counterparts, nuns were adept at administering their properties, exercising banal rights, and protecting their interests, both politically (in ecclesiastical courts) and symbolically (through the manipulation of histories, relic cults, sacred processions, and the liturgy).

After 1150, Johnson rightly maintains, the situation began to change. Both monks and nuns experi-

enced institutional and moral decline, in large part because the laity shifted its support to more urban religious groups such as the friars. But nuns suffered more than monks, because people now preferred intercessory masses to monastic prayers: monks could adapt to this change by becoming priests, nuns could not. Johnson also links the decline of female monasticism to a shift from noble to bourgeois culture. Unlike nobles, who granted some equality to women by virtue of their birth, the bourgeoisie widened the division between the public and private spheres, confining its women to the private.

I like Johnson's account of what nuns did, but I am uncomfortable with a portrayal of religious life that largely ignores intellectual productivity. I also disagree with her chronologically compressed account of the social and religious consequences of urban growth.

Johnson mentions that some convents had schools, some nuns knew Latin, and some even wrote Latin poetry. But her examples are hardly impressive in light of the intellectual and literary achievements of male Cistercians in twelfth-century France. She might have attempted to explain why French Cistercian nuns did not participate in this cultural flowering, or why there were no female monastic authors in France whose works could match those of Hildegard of Bingen, Elisabeth of Schönau, and the nuns of Helfta.

During the thirteenth century, male and female Cistercian houses in the lowlands (including one house that Johnson studied) supported a new cultural movement—that of the Beguines—which was literary, female, uncloistered, and urban. Bl. Juliana of Cornillon appears in the pages of Johnson's book, but we are never told that Juliana was part of this movement, or that the percentage of female saints doubled in the thirteenth century. The Beguines produced the first works of literature in the Walloon and Middle Dutch dialects, and some of those writings are still considered literary classics. Moreover, Juliana herself was largely responsible for the Catholic church's adoption of the feast of Corpus Christi. Johnson might lead us to believe that the streets of High and late-medieval cities were devoid of women, and that women had no economic freedom, spiritual voice, or influence in that period, but this was hardly the case.

SHARON FARMER
University of California,
Santa Barbara

ESTHER COHEN. *The Crossroads of Justice: Law and Culture in Late Medieval France*. (Brill's Studies in Intellectual History, number 36.) New York: E. J. Brill. 1992. Pp. x, 132. \$65.71.

Esther Cohen takes it as obvious that law in northern France in the late Middle Ages maintained its persua-

sive power because a wide variety of cultural practices outside or on the margins of standard legal procedure paralleled those within it. Concentrating largely on what many might consider *outré* "punitive rituals" employed against Jews, women, animals, and the dead, Cohen describes the parallels between these rituals and regular judicial procedures and punishments and explores the interpenetration of the various types of culture in which punitive rituals were enacted. These parallels and connections "might have seemed most trivial" to medieval lawyers themselves, Cohen argues, "but to historians [they] carry great import" (p. 2). The ultimate aim is to see how apparently eccentric cruelties and atavistic ceremonies were part of "one cultural unity" (p. 3).

Cohen has insisted before ("Law, Folklore and Animal Lore," *Past and Present* 110 [February 1986], 6–37) and repeats here that in Europe animals were put on trial for felonies in the Middle Ages. This is one of the key arguments of her book, for she sees dramatic parallels with—and inversions of—conventional trials in these procedures. Yet she depends on a number of old and sometimes curious books for her evidence, the vast majority of which come from legal *consilia*. Nowhere, however, does she address the nature of *consilia* as sources.

Consilia could be either selective memoirs of cases (as Cohen considers them) or model cases. They could include deliberately bizarre cases, bringing out wonderful points to help would-be lawyers learn or young lawyers perfect their craft. They parallel model books in Renaissance England from which, for example, one might learn the best arguments to use to invalidate "the bond of a pound of flesh" (that is, any horrendous contract), without anyone believing that such a case necessarily had taken place. Cohen, of course, believes that her animal trials did take place, but she admits that there "is no evidence that anywhere in France the animal in question was actually brought into court" (p. 111). Unfortunately, further into the argument she forgets this judicious admission and in a heated passage writes of the spectacular inversions that took place when "flesh-and-blood animals were placed in a human position on the dock and tried for homicide" (p. 175).

It must be said, too, that Cohen's case largely depends, in this instance at least, on her precise choice of words to translate her texts. If she systematically used "impounded," as she occasionally does, for "arrested," "investigated" for "tried," and "killed" for "executed," most of her animal trials might vanish into thin air.

In a short review it is not possible to treat every one of the practices that Cohen discusses. I have concentrated on her case for the animal trials partly because it is a centerpiece of her argument and partly because the problems of the argument are most obvious here, but the treatment of nearly every other issue raises equally numerous, if sometimes different, questions in my mind. Nonetheless, if a book is to be judged by

its power to make one challenge and rethink old beliefs—if only in the end to reaffirm them—then this book serves a useful purpose.

WILLIAM CHESTER JORDAN
Princeton University

ROBERT CHAZAN. *Barcelona and Beyond: The Disputation of 1263 and Its Aftermath*. Berkeley and Los Angeles: University of California Press. 1992. Pp. x, 257. \$40.00.

Between July 20 and 27, 1263, under the auspices of the king and the Dominican friars of Aragon, before a grand assembly of courtiers, churchmen, and Jews in Barcelona, Friar Paul Christian engaged Rabbi Moses Nahmanides in debate over the fundamental beliefs of Judaism and Christianity. Surviving eyewitness accounts of the event, in a relatively brief Latin protocol of the Dominicans and a considerably longer Hebrew narrative by Nahmanides himself, have rendered it one of the most well-known, colorful, and oft-discussed examples of religious disputation in the Middle Ages. For well over a century, historians of medieval Jews and Christians have analyzed the texts and the incidents precipitating them from a variety of perspectives. Late in the nineteenth century, scholars such as Heinrich Denifle and Isidore Loeb weighed the naturally conflicting evaluations of Latin and Hebrew reports and appeared to reenact the jousting of medieval theologians in the guise of modern scholarly discourse. Approaching the turn of the century, Solomon Schechter ("Nachmanides," in *Studies in Judaism: First Series* [1896], 104) cynically doubted "that there is in the whole domain of literature less profitable reading" than polemical texts such as these. In more recent years, numerous investigators have reaffirmed the value of these medieval reports for more dispassionate study of Jewish-Christian relations in particular and of interreligious polemic in general. Linking other thirteenth-century events and documents to the Barcelona debate or to the elusive but fascinating career of Friar Paul (the erstwhile Jew, Saul of Montpellier) has become fashionable among historians, and a recent British television dramatization of "The Disputation" has proved popular in a range of diverse settings, from the American college classroom to the halls of an antimissionary organization in Jerusalem. Robert Chazan has himself dealt with the disputation and its participants in portions of two previous books and at least half a dozen articles. Yet noting (p. 205) the comment of a reviewer (Norman Roth) on his preceding monograph that "more remains to be said" on the subject, Chazan has here devoted a book-length study to the events, the written records, and the historical significance of the disputation of Barcelona.

For the purposes of this brief review, one might conveniently divide Chazan's book into three parts. Following an introduction that surveys the existing historiography and outlines methodological princi-

ples, chapters 1 and 2 concern the setting and actual play-by-play of the debate. True to his conviction that a careful analysis of even polemical narrative can yield accurate reconstructions of medieval events, Chazan presents an admirably balanced summary of the arguments reportedly exchanged by Friar Paul and Rabbi Moses in Barcelona. Yet much in this section has already been said, by Chazan and by others, and the author's appeal to "recent social science literature and literary criticism" (p. ix) notwithstanding, the very notion that such literature of religious polemic does contain the prized "kernel of truth" of what happened in fact remains questionable. Chapters 3 through 5 move beyond the events to the texts of the disputation. Here, too, much has been said before, and the analysis of the lengthy Nahmanidean narrative is at times so painstaking as to lose its reader's focus. The underlying purpose of these chapters, however, proves fruitful and refreshing, inasmuch as their accent falls precisely on the distance between text and event, and on the masterful techniques employed by Nahmanides to address pressing issues at the moment of inscription. Finally, chapters 6 and 7 address two other—and later—Nahmanidean texts: a commentary on the suffering servant passages in Isaiah and a treatise on the redemption. Seeking to integrate the concerns of Nahmanides the polemicist with other dimensions of his world view and literary corpus, these chapters open new and important issues in assessing the impact of Barcelona on Rabbi Moses himself and on the cultural history of thirteenth-century Spanish Jewry. Here I wished that the author would have pursued his inquiry at greater length, even at the expense of shifting his book's emphasis away from the disputation.

Chazan's study benefits from more than two decades of research on the events, characters, and memories of a fascinating historical occurrence. Within the covers of a single volume, which includes thorough documentation and bibliography, it offers a comprehensive review of its subject that will undoubtedly prove to be the starting point in most subsequent study of the debate. Its notes make reference to two forthcoming monographs by its author, on the decline of early Ashkenazic Jewry and on the book of Daniel in medieval Judaism, which readers should eagerly await.

JEREMY COHEN
Tel Aviv University
Ohio State University,
Columbus

MARIA THERESA FERRER I MALLOL. *Organització i defensa d'un territori fronterer: La Governació d'Oriola en el segle XIV*. (Anuario de Estudios Medievales, number 22.) Barcelona: Consell Superior d'Investigacions Científiques. 1990. Pp. xxx, 631.

Making extensive use of documents in the Arxiu de la Corona d'Aragó in Barcelona, the Archivo Historico Nacional in Madrid, various local archives, and printed collections of documents, Maria Theresa Ferrer i Mallol presents a detailed—perhaps overly detailed—study of the history of a strip of territory that became known as Oriola between 1296 and 1410. Appended are 270 documents or summaries of documents. There are also three line-drawing maps of use only to those familiar with the region's geography.

The region was part of the kingdom of Murcia conquered by Castile. When Alfonso the Wise of Castile violated his own law by denying the throne to the eldest son of his dead eldest son and proclaiming his second son heir, the children of the eldest son fled to Aragon for protection. Encouraged by both Castilian and Aragonese nobles as well as Catalan merchants, the children plotted their restoration and became pawns in the hands of their supporters.

The power struggle between king and nobles within Castile permitted the Aragonese-Catalans repeatedly to violate treaties that limited Aragon's Iberian expansion. The aggressive territorial ambitions of the Aragonese lords and the commercial desires of the Catalan merchants led them to view the Murcian kingdom as a ripe field for expansion. The crown's sovereign, Jaume II, having difficulties with his brother, the king of Sicily, and his cousin, the king of Mallorca, had no desire to engage in a full-scale war over Murcia. He accepted a compromise granting Aragon a sliver of Murcian land eventually called Oriola, which was de jure, but not de facto, annexed to the kingdom of Valencia, one of the Iberian components of the crown. While some Aragonese lords felt cheated by the compromise, the Catalans accepted it because they secured economic privileges in the remaining section of Murcia.

Ferrer i Mallo omits most of this background because she assumes that all her readers know it. She examines the political, social, and economic (but not religious) developments in the region, including a detailed study of all officials, recipients of land grants, castles, spies, and so forth. Chapter 7 provides a major, but limited, discussion of the Almogàveres' activities in Iberia.

The bibliography, footnotes, and index range from excellent to helpful. There are two slight problems. Ferrer i Mallol, who has published some nineteen studies on the region, assumes that the reader has sufficient background to place Oriolan events in a broader context. The result is that the reader at times feels that the work consists of isolated vignettes rather than a fleshed-out study. Some might find irony in another problem I have with this book. Having insisted in my own works on the use of Catalan orthography where appropriate (for example, Jaume, Pere, and Catalunya instead of James, Peter, and Catalonia, or Jaime, Pedro, and Cataluña), I find it irresponsible to use modern Catalan to replace abbreviated Latin or medieval Catalan in transcribing documents. Fur-

thermore, it seems an error to use modern Catalan proper names in the spelling of either Castilian or Sicilian names. There is an attempt in the index to correct this problem by listing some alternatives after the Catalan version. But the effort is hit-and-miss, and unless one knows the current Catalan spelling, seeking individuals or families is difficult.

Notwithstanding these limitations and difficulties, this work will be of use to a select group of specialists.

J. LEE SHNEIDMAN
Adelphi University

WERNER RÖSENER. *Grundherrschaft im Wandel: Untersuchungen zur Entwicklung geistlicher Grundherrschaften im südwestdeutschen Raum vom 9. bis 14. Jahrhundert*. (Veröffentlichungen des Max-Planck-Instituts für Geschichte, number 102.) Göttingen: Vandenhoeck und Ruprecht. 1991. Pp. 607. DM 122.

Werner Rösener describes the transformation of agrarian life in southwestern Germany from a manorial-based lordship over land and people to a rent-based system of lordship as it occurred on selected ecclesiastical estates. The study begins with a discussion of *Grundherrschaft* and its use by jurists, sociologists, and historians. *Grundherrschaft*, as opposed to the Latin *dominus fundi*, was first used in the sixteenth century, and analysis of the term has concentrated on its debt to German and Roman legal and constitutional traditions. Rösener prefers to discuss it as an economic system that could vary by time and place.

The second section of the book is a careful reconstruction from the extant sources of the experiences of old imperial abbeys and bishoprics situated on both sides of the Rhine, in Swabia, Alsace, and Ratia. These institutions are ideal subjects because they allow the investigation of economic and administrative patterns from the ninth through the twelfth centuries. Rösener's method is to establish a general pattern through the investigation of the Abbey of Weissenburg, founded in 661. Then he adds shorter investigations of less well-documented institutions like the abbeys of Marmoutier, St. Gall, and Einsiedeln and of canonries and local churches in Constance, Schaffhausen, and Basel. Through a careful deconstruction of Weissenburg's late-thirteenth-century *Liber possessionum*, which recounted the earliest claims to particular lands and jurisdictions of the abbey, Rösener is able to separate "old" and "new" lands on which the process of change was remarkably different. The older lands, often dispersed and inefficient to manage, tended to be rented out earliest. Those lands Weissenburg acquired in the eleventh or twelfth centuries tended to be smaller, more compact estates that the abbey continued to manage directly as long as possible. The pattern was largely similar in the other institutions. Thus, the transformation to a rent-based agriculture was a complex one begun as

early as the ninth century and continuing into the twelfth; it varied by time and place.

In the final section Rösener discusses the impact of the transformation on villagers and their landlords. Abbeys and canonries, he concludes, were efficient landlords. They participated in the agricultural innovations of the period. They cleared land, sold off distant estates, and introduced new crops and techniques. The most significant changes, however, occurred in the villages. Labor services, where they continued, were based on membership in the village community rather than personal status. Furthermore, the old estate managers were replaced by ministerials, the bonded officials who came to dominate public administration in Swabia. And finally, as in many other parts of Europe in the twelfth and thirteenth centuries, the reorganization of peasant holdings brought an increasingly complex social differentiation in the villages. Rösener concludes that the transformation of the *Grundherrschaft* in the High Middle Ages was a transformation comparable to German agrarian reforms of the eighteenth and nineteenth centuries.

DUANE J. OSHEIM
University of Virginia

MARKUS BITTMANN. *Kreditwirtschaft und Finanzierungsmethoden: Studien zu den wirtschaftlichen Verhältnissen des Adels im westlichen Bodenseeraum 1300–1500*. (Vierteljahrsschrift für Sozial- und Wirtschaftsgeschichte, Beihefte, number 99.) Stuttgart: Franz Steiner. 1991. Pp. 303. DM 88.

Markus Bittmann's book, originally a dissertation for the University of Constance, investigates the economic behavior of the late-medieval nobility of the West Bodensee region. Although the economic practices of townspeople are well known, Bittmann claims that we are just beginning to understand the economic mentality of the nobility. This study is based on urban and regional archival sources as well as published materials. Bittmann situates his inquiry in the context of recent German scholarship. He attempts some regional comparison but limits his vision narrowly to the Germanic world.

For specific analysis Bittmann has chosen fourteen noble families of the West Bodensee area on the basis of criteria such as length of residence. He sees the West Bodensee as a trouble spot in social and political terms during the fourteenth and fifteenth centuries and thus an appropriate arena in which to raise questions about noble financial strategies. Theoretical and methodological discussions of topics such as financial management, currency problems, and credit instruments precede empirical sections. This is not a statistical study, given the lacunae of the sources, particularly for the fourteenth century.

Salaried service outside the feudal context was one

means of augmenting declining traditional noble landed income in the late Middle Ages. The Bodensee nobility sought employment in military, or in some cases administrative, capacities primarily from royal (especially King Sigismund) or ducal (Austria) circles, but occasionally from the bishop of Constance, from other regional nobility, and from foreign sources such as the Italian towns.

War and feud also offered possibilities for income enhancement, particularly in the areas of booty and ransom. *Raubrittertum* could be a lucrative occupation, but gains had to be weighed against war injuries and arms expenditures. Complicating the political situation in this regard was the growing antagonism between German towns and the nobility. Although some expeditions could be extremely lucrative—an attack on merchants yielded 120,000 *Rheingulden*—in general, war and feud were losing transactions in economic terms.

Financial options expanded as the mortgage evolved from the old direct right of use over the mortgaged object to the receipt of fixed interest for a fixed term. With the expansion of the capital market came the implementation of the more modern financial instrument of the rent. Towns of the Bodensee area—Lindau, Schaffhausen, Zürich, Basel, Ulm, and Nürnberg—furnished rent credit.

Marriage offered a possibility for fortune building. Bittmann provides a chart detailing the marriage arrangements of individuals from his group of fourteen families, with the amounts of *Heimsteuer*, *Morgengabe*, and *Widerlegung*. Only rarely were the financial motives behind marriage alliances clearly detailed, and the best designs fell on bad luck, as in the case of Ulrich von Hohenklingen, the last of his line, whose rich marriage produced no children. By the same token, provisions for numerous daughters and sons could put much financial stress on a family.

The somewhat scattered details of the fourteen noble families' economic activities left me wishing for a prosopographical study of the Bodensee nobility to provide historical context. More systematic commentary on the fate of the families over the course of these two centuries—some declined, some were enriched—would be helpful. The author has succeeded in revealing a complex web of factors—political, financial, and individual—at play in the evolution of noble family fortunes.

KATHRYN L. REYERSON
University of Minnesota,
Twin Cities

JOACHIM BUMKE. *Courtly Culture: Literature and Society in the High Middle Ages*. Translated by THOMAS DUNLAP. Berkeley and Los Angeles: University of California Press. 1991. Pp. ix, 770. \$55.00.

This book is a translation of Joachim Bumke's *Höfische Kultur: Literatur und Gesellschaft im hohen Mit-*

telalter (1986). Contrary to the possible implications of either title, however, Bumke concentrates on the evolving nature of such a culture only in Germany (although with some French antecedents), not in Europe as a whole. Moreover, the book is a product more of compilation and synthesis than of original research. As Bumke states, "This essay is not addressed to specialists. It is in the nature of an introduction, and is meant to be accessible to readers without specialized knowledge" (p. 17).

Nevertheless, both literary and historical specialists are likely to find this book rather more useful than Bumke appears to have imagined. If one is concerned with the relationship between literature and life, the scholarship is so extensive that keeping up with it presents a well-nigh impossible task. The great virtue of this study is that it brings together in one accessible place the most recent findings of modern research. As a result, readers will come away from its pages with a much clearer sense of what we now know about medieval German courts, the literature produced both in and for them, and the ways, if any, artistic vision influenced courtly behavior—and vice versa.

Starting with basic concepts of the social order, Bumke moves on through an impressive range of topics, from palaces and their furnishings to luxurious tents, from dress codes and precious fabrics to knightly weapons and their social significance. His principal focus, however, is on the courtly feast, not daily life, since it is one of his basic contentions that feasts and festivals provided the occasion, if not clearly the cause, for the production of courtly literature both epic and lyric. Although many literary examples are introduced throughout, until the final chapter the emphasis falls primarily on real courts and real people, not on the inventions of literature. Indeed, even the last chapter, "The Literary Scene of the Courtly Age," remains essentially historical, for even though Bumke deals with a variety of literary issues, he still tends to stress concretely verifiable facts about authors and their social backgrounds, patronage, and the nature of the courtly audience insofar as these things can be known.

Bumke's conclusions are many, various, and, given the nature of his synthesizing approach, not entirely new. Nevertheless, at least four of them bear repeating. First, it becomes clear just how dependent German courtly literature initially was on developments in France that had taken place at least fifty to one hundred years earlier. Second, a necessary precondition for the appearance of a viable courtly literature was not just a reasonable level of literacy among the nobility but also a reasonably well-developed chancery at the court where such literature was enjoyed and patronized. Third, Bumke's analysis underscores the extent to which different kinds of patronage were involved for lyric and epic poetry. After all, lyrics tend to be short and are quickly written, so they do not require extended support, whereas the composition of an epic could require years, thereby necessi-

tating a wholly different level of support. Indeed, Bumke speculates that many epics remain incomplete not because their authors lost interest or simply died, but because their sponsors ceased their patronage.

Fourth and last, however, Bumke's most frequent statement throughout the book is a variant on "This development still requires historical documentation." What he makes clear, then, is just how much remains to be done if we are truly to understand literature and society in the High Middle Ages. Some is impossible to do, of course, given gaps in the evidence, but it looks to me, at least, as though there are enough worthwhile questions that can be answered to provide research topics for years to come.

CHARLES T. WOOD
Brown University

TONY HUNT. *Teaching and Learning Latin in Thirteenth-Century England*. Volume 1, *Texts*; volume 2, *Glosses*; volume 3, *Indexes*. Rochester, N.Y.: Boydell. 1991. Pp. ix, 453; 173; 364. \$199.00 the set.

The title of Tony Hunt's extremely learned work does not adequately reflect the content and nature of his three volumes. The reader will not find here a study of medieval pedagogy, of Latin instruction in the grammar schools of thirteenth-century England, or of Latin literacy during the reigns of Henry III and Edward I. Instead, Hunt's work is a detailed study of the vernacular glossing of Latin texts and terms up to the thirteenth century, a vast compendium of erudition in the tradition of R. W. Hunt's *Collected Papers on the History of Grammar in the Middle Ages* (1980), of Minio-Paluello's *Twelfth-Century Logic*, 2 vols. (1956, 1958), or L. M. De Rijk's *Logica modernorum* 2 vols. (1962, 1967). Most of the work is devoted to descriptions of the manuscripts containing vernacular (primarily Anglo-Norman) glosses of Latin texts, editions of the most important glosses, and the creation of a glossary.

Hunt's first volume begins with a brief but detailed overview of Latin and vernacular glossing from the Hellenistic period to the thirteenth century. Then, moving manuscript by manuscript, it gives examples of glossing in the Anglo-Saxon and early Norman periods for texts in literature, liturgy and hymnology, botany, and law. This is followed by chapters on the glossing of classical authors, grammar texts, twelfth-century wordbooks (specifically those of Adam of Balsham [Parvipontanus], Alexander Nequam, and John of Garland), integrated glosses, specialized glosses (exotica), and dictionaries. Volume two contains the interlinear glosses corresponding to the chapters of volume one. Volume three consists of two indices: Latin to vernacular, and vernacular to Latin.

The work will be particularly useful to scholars working in medieval literature (both Latin and vernacular), and especially to those interested in French and Anglo-Norman philology and lexicography. It

will also be of interest to the growing body of scholars working in the field of medieval grammar. Less directly, but no less important, there is abundant (if undigested) material here for those who wish to understand how Latin was acquired and studied in the schools of England and northern France in the thirteenth century, and how thirteenth-century scholars, through improved grammars and glossing, sought to bridge the gap between Latin literacy and vernacular usage—both the process of teaching Latin and the process of translating for those who did not have Latin skills.

WILLIAM J. COURTENAY
University of Wisconsin,
Madison

HEATHER SWANSON. *Medieval Artisans: An Urban Class in Late Medieval England*. New York: Blackwell. 1989. Pp. 189.

The most valuable part of Heather Swanson's book is probably its systematic and thoughtful survey of different crafts and industries, including some that have been relatively neglected, like the victualing and clothing (as opposed to textile) trades and the building industry seen as a whole. The evidence comes primarily from York but comparative material is also drawn from other relatively large English towns, although London, small market towns, and rural artisans are omitted (and the index does not cover all the references to other towns). It is the book of a documentary historian rather than an archaeologist. It covers a shorter period and contains less technical detail than J. Blair and N. Ramsay, eds., *English Medieval Industries* (1991), but it has much more about the organization of work, the economic fortunes of different trades, the relations among artisans, and between them and their municipal rulers.

Swanson's principal arguments are that the extension of the franchise in late-medieval towns was connected with the growing polarization of merchants and artisans, and that both of these were connected with the deliberate extension of control over the crafts by municipal governments in the interests of the mercantile elite and with the deliberate exclusion of artisans from government. Some of her arguments are not entirely free from circularity. The contention that merchants and artisans formed sufficiently separate and cohesive classes for their conflicting interests to have determined so much municipal policy seems at times to be a premise rather than an argument to be proved. Deducing intentions and policies from what we find in medieval documents is always difficult and needs perhaps a bit more attention to ideas and norms that seem to have been generally accepted throughout the Middle Ages. Government control of trade and industry seems to have been universally accepted in principle, as was the idea that controls were supposed to be imposed in the

interests of all, but that the elite should, with the advice and consent of the solid citizens among the rest, decide what those interests were. But medieval governments, however authoritarian in principle, always left much of the actual work of applying the rules to people lower down. They also sometimes had to respond to pressure from below. Swanson's reading of the York records may slightly overplay both the power of municipal governments and their deliberately oppressive and exclusive policies. The mayor and council's abolition of a fee is dubious evidence of their "absolute control of the system" (p. 119). As she says (p. 113), craft ordinances "are very ambiguous documents; they express both the interests of the crafts and the preoccupation of the authorities." Artisans, however discontented with corruption and what they saw as injustice, did not always object to what we would think they should have objected to. Like most historians who have quoted the claim in a York petition of 1475 that all members of the "commonalte" who had not held office were "unlike prevaliged," Swanson does not mention that this led to a request that city chamberlains should be appointed only from among former bridgemasters and bridgemasters only from "the most able men in goods and discretion." But these are subjects that need argument, and they are well argued here. The book deserves close attention both for its scholarship and the common-sense clarity of its presentation. The subject of late-medieval urban industry is now raised to a new level of discussion.

SUSAN REYNOLDS
London, England

CHRISTINE CARPENTER. *Locality and Polity: A Study of Warwickshire Landed Society, 1401-1499*. New York: Cambridge University Press. 1992. Pp. xviii, 793. \$125.00.

The most recent generation of English medieval historians has turned its attention from high politics and constitutional development to social and economic questions. Recent studies have necessarily focused on local history, on the economics, social structure, and politics of localities, or on the fortunes of individual families. Christine Carpenter's work draws all these themes together in a large-scale collective history of the Warwickshire gentry over a whole century.

This book differs from many earlier studies not only in its scope but also in its emphasis on the lesser landholding aristocracy, the knights, esquires, and above all on those smaller landholders who constituted the newly defined class of gentlemen. It embraces the fortunes, economic, social, and political, of nearly 900 individuals. The study rests on a massive foundation of documents drawn from the public archives of the law courts as well as the private records of individual families, wills, deeds, and estate

accounts. There are useful statistical tables to summarize data, but the book is given a greater richness and added liveliness by abundant use of particular family histories. Sometimes in the telling of these multiple histories the trees become very thick, but Carpenter never loses sight of her main argument.

The first section of the book, concerning the structural problems of status and economy, begins with the key question as to who the gentry were. Contemporaries were still uncertain how to place the large body of men who were lords of land and tenants, clearly *gentil*, yet who lacked the traditional qualifications of a knight or esquire. Carpenter's account of the process by which, in the early years of the fifteenth century, a line was drawn that defined their place in the elite, illuminates not only the local society of Warwickshire but also the larger national landscape. Carpenter details *in parvo* a process of social definition that shaped English society for generations to come.

The chapters that follow examine exhaustively the evidence for the economic fortunes of a class for whom land and status were inextricably bound together. Agrarian depression and acute political instability from 1450 to the 1490s made management of their resources difficult. What the record reveals is their toughness and flexibility in preserving intact family estates and insuring the continuity of the line. The evidence presented here goes far to demolish the thesis that this was an age of social mobility, foreshadowing the upward push of the sixteenth century. Warwickshire society in the fifteenth century is revealed as little changed in personnel or structure. Upward mobility was rare, and few newcomers made their way into its ranks.

This carefully substantiated picture of the socioeconomic framework is prelude to the larger second section of the book, what Carpenter terms a sociopolitical history of the Warwickshire gentry from the rise of the Lancastrian house to the end of the first Tudor's reign. The theme underlying this dense and detailed narrative is the struggle of each landed family to preserve its inheritance and its elite position in a world shadowed by the continual threat of violence, and where the law was the instrument of those powerful enough to use it. Public peace and private property were maintained by informal local networks of association usually centered around a local nobleman. Complex in structure, often overlapping in personnel, the networks shifted with the circumstances of time and mortality. Their effectiveness rested on the preservation of public order and private security in Warwickshire.

Carpenter has two original and important points to make about these networks. First, they were not vertical but rather horizontal, not noble clientages but rather unions of cooperation, grouped around a leader. Second, over the century the role of the nobility in these groupings faded, so that by 1500 social and political equilibrium in Warwickshire was

maintained by the non-noble landed aristocracy. It was with them that the Yorkist and Tudor monarchs had to deal in governing the country.

In Warwickshire the lesser aristocracy won for itself coequal membership in a trinity of power, alongside nobles and the monarchy. This in turn posed a new question. These semiautonomous gentry networks, viable but fragile, required from time to time the firm hand of an umpire. There was no longer any regional magnate in Warwickshire available to fit that role; only the monarchy could fill it. In Carpenter's account, the crown in 1500 was still fumbling uncertainly in the management of this relationship. In her view the inability of Henry VII to grasp the nature of the problem posed the risk of serious disequilibrium. The book therefore ends on a note of questioning uncertainty. How were the links forged that joined the later Tudor monarchs in an articulated system of reciprocal trust, cemented in a hierarchical structure of patronage? Carpenter has posed some awkward and demanding questions for the historians of the sixteenth century to answer.

Carpenter has deployed a massive array of descriptive and narrative material and skillfully blended it into an incisive and sustained argument. This is an important book, not only for specialists in the later medieval period and the Tudor age but also for those seeking to understand the process that created a social structure so tough and so enduring that it lasted from the age of Sir Thomas Malory to that of George Eliot.

WALLACE T. MACCAFFREY
Harvard University

T. H. LLOYD. *England and the German Hanse, 1157–1611: A Study of Their Trade and Commercial Diplomacy*. New York: Cambridge University Press. 1992. Pp. ix, 401. \$79.95.

T. H. Lloyd has already established an enviable reputation with two impressive books on medieval English foreign trade: *England's Wool Trade in the Middle Ages* (1977) and *Alien Merchants in England in the High Middle Ages* (1982). Drawing on a considerable amount of additional research covering four centuries, Lloyd has produced another important monograph in what is evidently the first complete study on Anglo-Hanse commerce, from genesis to nemesis. The book is organized chronologically in six chapters: 1157[sic: should be 1175]–1361, for the acquisition of Hanse privileges in England; 1361–99, for the growing English commercial challenge in the Baltic; 1400–37, for increasingly tense Anglo-Hanse relations culminating in the Vorrath treaty; 1437–74, for the era of conflict culminating in the supposedly disastrous Utrecht treaty; 1474–1551, for Anglo-Hanse rivalry at the Antwerp market; and, finally, 1551–1611, for the Hanse's piecemeal loss of their English privileges, the loss of the war-stricken Ant-

werp market, and the English acquisition of the Hamburg staple. The first half of each chapter is devoted to diplomatic, political, and military relations; the second half to changing trade patterns. Had Lloyd instead commenced each chapter with his commercial analyses, often quite insightful (especially for 1370–99), I would have gained a better understanding of the diplomatic relations.

Chapters 2–4, on the struggle over the Baltic from 1361 to 1474, constitute Lloyd's major contribution. Thanks to the publications of Michael Postan, European economic historians have long appreciated the importance of that struggle, although it is treated far too superficially in Philippe Dollinger's widely used *The German Hansa* (1970). Postan's thesis was that both the English and the Dutch began entering the Baltic in the late fourteenth century, after contracting European markets had become "depressed," thus provoking a hostile reaction from the Hanse; that the English, focusing on Prussia, saw the apogee of their gains in the Vorrath treaty of 1437; that crown-sponsored English piracy then united both Lübeck and the Prussian towns in an implacable resolve to evict the English from the Baltic, a goal achieved in compelling Edward IV to accept the humiliating Utrecht treaty (1474); that English merchants, having also lost direct access to markets in Scandinavia, Iceland, and France, were forced to canalize their overseas trade almost exclusively on Antwerp, thus precipitating its rise to glory in the later fifteenth century; that the Dutch then succeeded in vanquishing the Hanse and in gaining overwhelming control of the Baltic trades, so that when English ships reappeared there during Elizabeth's reign, they were unable even to challenge the Dutch hegemony, an early modern European commercial-financial hegemony based essentially on control of this Baltic commerce.

Lloyd challenges or modifies many points in this dramatic picture of "turning points." He demonstrates, first, that the English had both begun their expansion and achieved their major Prussian successes much earlier than Postan indicated, indeed with the Anglo-Prussian treaty of 1409; that Postan had overrated both the importance of the Vorrath treaty and its consequences, and also the crown's role in subsequent piracy; and that the Utrecht treaty, substantially similar to the Vorrath treaty, was no disaster. It had little real impact on Anglo-Prussian trade, which had long suffered a slow decline, while English commerce, from the 1420s, had been increasingly directed toward the emerging Antwerp market, for reasons quite unrelated to the Baltic Hanse. Finally, he argues that from the early sixteenth century, according to the Sound Tolls, English shipping was enjoying a respectable presence in the Baltic.

What I missed from this engaging analysis, however, was any discussion of Postan's "depression" thesis, of demographic and other structural changes in Baltic and European markets, of the crucial role of

the Dutch, and of the complex factors that explain the rise and then decline of the Antwerp market, a theme dominating the last third of his book. Indeed, his bibliography on Antwerp and the Low Countries is distressingly thin. I must also note that Stuart Jenks has recently published a far more deeply and widely researched three-volume study on the very core subject of Lloyd's book: *England, die Hanse und Preussen: Handel und Diplomatie, 1377-1474* (1992). This work is indispensable; but Lloyd's monograph is also one that no economic historian of late-medieval and early modern Europe can afford to miss.

JOHN H. MUNRO
University of Toronto

MODERN EUROPE

MICHEL DE CERTEAU. *The Mystic Fable*. Volume 1, *The Sixteenth and Seventeenth Centuries*. Translated by MICHAEL B. SMITH. (Religion and Postmodernism.) Chicago: University of Chicago Press. 1992. Pp. x, 374. \$35.00.

Michel de Certeau, one of the most powerful minds to come out of France in the postwar period, has written an original, rich, and decidedly challenging synthesis of sacred history, semiotics, and psychology. Grandly conceived and magnificently written, the work stands as an extraordinary contribution to the series *Religion and Postmodernism*.

But this is more directly a study in religion and modernism, the decline of mysticism and, at least by default, the rise of the Enlightenment. More specifically, it is about what Louis Cognet termed the "antimysticism" and T. S. Eliot "the dissociation of sensibility" of the later seventeenth century. If mysticism is about unity, this is about disjunction: the bifurcation of letter and spirit, language and meaning, the sign and the signified. The creative intelligence of Certeau could put it incomparably: "*Mystics* is the anti-Babel, the quest for a common speech after its breakdown, the invention of a language 'of God' or 'of the angels' that would compensate for the dispersal of human language" (p. 157).

Such comments can intimate that, although this synthesis has rigor, it can be allusively, even hauntingly, written. Certeau begins by positing a decline of mysticism in the later Middle Ages and a corresponding rise of eroticism, a kind of transference. "The One," as he puts it, "has changed its site." "But the adored body is as elusive as the vanishing God." Against this transition, the mystics are seen as engaged in "hand-to-hand combat" (pp. 4-5). But, to be sure, they are unable to stay the "ersatz presence" of modernity ascendant by the middle of the seventeenth century.

The focus of this volume is the critical period from St. Teresa to Madame Guyon. Certeau relates the melancholy story of the posthumous editing if not domestication of the mystical doctor, St. John of the

Cross (d. 1591), by the succeeding generation of Carmelites. Of course, John died in bad favor with the Carmelite authorities, who pushed St. Teresa, who was irrepressible in her own right but less universal and more devotional. This made her therefore more congruent with the Counter Reformation and "modern" spirituality. Against the background of the rise of Protestantism and the rash of *illuminati*, the watchwords were moderation, prudence, and obedience. Certeau traces this reaction within the Society of Jesus and goes so far as to speak of "the treason" of the clerics.

A consequence of this failure of nerve, of course, was, despite baroque luminosity, a certain alienation between mysticism and the church, with both mystical and institutional losses. Since any vacuum would be filled, the author argues for the ascendancy of "the wildman," the "enlightened illiterate," and for the woman's "body" as being "exegeted" by the learned, professional theologian. The issue is the separation of theology and experience.

The climactic case study is that of "Labadie the Nomad." The label "Nomad," to be sure, is appropriate geographically, confessionally, and, maybe, psychologically. This Jesuit turned Jansenist turned Calvinist turned Pietist turned Millenarian turned, finally, "Labadist," a sect almost to himself, knew no coherence. He is decentered, and his Millenarianism is deemed a flight into the future. His inflated language is representative of "the unbinding of meaning." Certeau concludes of him: "He composed a kind of *junk mystics*, just as there is today a kind of *junk music*" (p. 290).

Rather than a review, Certeau's book ought to be subject matter for an entire graduate seminar—if someone could be found with the ability to teach it. Still, inclusive as it is, there may be some additions that could be made. For example, the pastoral crisis that cut across confessional frontiers bears materially on the decline of mysticism, that is, as the apologists had it, "the salvation of the people" displaced "the perfection of the few." Certeau has left us a splendid work, both intellectually and existentially challenging, and its impact should carry well over into the third millennium of Christianity. It should be read in conjunction with the likes of Stephen Toulmin's *Cosmopolis: The Hidden Agenda of Modernity* (1990). Certeau reinforces the disturbing question as to whether modernity is about autonomy or alienation.

D. CHRISTOPHER NUGENT
University of Kentucky

NANCY S. STRUEVER. *Theory as Practice: Ethical Inquiry in the Renaissance*. Chicago: University of Chicago Press. 1992. Pp. xiii, 246. \$32.50.

Nancy S. Struever wants to demonstrate that Renaissance ethical theory is relevant to a number of contemporary philosophical issues. According to her, the

reason why this era has not previously received due attention is the historical approach taken by scholars such as Quentin Skinner, who are so concerned to avoid the sins of presentism and anachronism that they end up producing a "debilitating antiquarianism" (p. xi). She is equally dismissive of those who take a Whiggish attitude, seeking in the past anticipations of modern developments. Instead, she argues, one should use the past to "gloss the present debate" (p. x) and look at problems raised by Ludwig Wittgenstein, W. V. O. Quine, J. L. Austin, Bernard Williams, and other twentieth-century philosophers in the light of the innovative thought of Petrarch, Nicolaus Cusanus, Lorenzo Valla, Machiavelli, and Montaigne.

Her decision to focus on these five Renaissance philosophers rests on certain shared characteristics that she perceives in their intellectual outlook: all were anti-academic, hostile to or alienated from the prevailing Aristotelian culture of the universities; all recognized the messy intractability of ethical issues, rejecting tidy systematizing and easy moralism; and, most importantly for her thesis, all concentrated on the nature of ethical inquiry itself, making advances in theory by examining investigative practice.

Part 1 deals with Petrarch and his relocation of inquiry. According to Struever, Petrarch removed the discussion of ethical questions from the scholastic treatise, which she (following him) characterizes as abstract, formal, and rigid, and treated them instead in the intimate, spontaneous, first-person humanist letter, with its concrete specificity, discursive informality, and avoidance of authoritative demonstration.

Part 2, entitled "Internal Discipline," concerns Cusanus and Valla, both of whom attempted to revise the language of inquiry, removing it from the exclusive control of an "unproductive professionalism" (p. 62) and making it accessible to the layman. Cusanus achieved this through his use of symbols, above all mathematical figures, which he manipulated in order to demonstrate that human knowledge was limited to the "domain of sign and appearance" (pp. 79–80). Valla's aims were linguistic rather than semiotic: he attacked the "overreaching lexicon and syntax of [the] Aristotelian program" (p. 96) and substituted for it a rhetorical grammar, solidly based on popular usage and ordinary language. Although Struever praises Valla's conviction that investigational discourse should be perspicuous, her own prose style displays a murkiness and jargon-ridden obscurity that could easily rival the most benighted product of late scholasticism.

"External Address" is the subject of part 3. Machiavelli and Montaigne, Struever maintains, carried through Petrarch's relocation of inquiry by working out strategies for addressing not the public world of university intellectuals but rather the private realm of those concerned with the problem of giving good counsel. Machiavelli responded to the challenge by developing a "problematizing strategy" (p. 148). He

abandoned the linear structure of events and adopted a hypothetical, subjunctive mode of narration, weaving tissues of alternate courses of events that revealed the innate complexity of any scenario. Like Machiavelli, Montaigne eschewed facile moralizing prescription and "unproblematic exemplary instances" (p. 202); like Petrarch, he adopted a highly personal, self-revelatory tone: inquiry, for him, was private, introspective, a conversation with himself.

Struever is well aware that the five figures she deals with are by no means representative of the spectrum of Renaissance ethical thought. It is their very unrepresentativeness, their persistent efforts to go against the academic grain, that makes them interesting to her, and useful, she believes, for contemporary philosophers. This perspective, however, severely limits the value of her study in historical terms. Struever justifies the narrowness of her approach by decrying, again and again, "the pervasive dysfunction in late Renaissance investigative modes" (p. 135). But dysfunctional or not, the overwhelming bulk of moral philosophy in this period was written by university-based Aristotelians, and no account that blithely dismisses this weighty and largely unexplored mass in order to concentrate on a handful of well-known texts can give us a valid picture of ethical inquiry in the Renaissance.

JILL KRAYE
Warburg Institute,
London

IAN MACLEAN. *Interpretation and Meaning in the Renaissance: The Case of Law*. (Ideas in Context, number 21.) New York: Cambridge University Press. 1992. Pp. xiii, 237. \$49.95.

Ian Maclean's wide-ranging and probing study explores how Renaissance jurists understood the task of legal interpretation and what explicit or implicit theories about language, signification, and meaning informed their undertaking. As the author observes, much controversy has arisen in recent intellectual history of the Renaissance regarding semantic and philosophical issues of language, especially in interpreting the writings of Renaissance grammarians, philosophers, and theologians. To this ongoing debate the case of legal studies has much to offer, Maclean suggests, for the Renaissance study of law possessed its own institutional contexts and intellectual traditions. University law faculties deployed a distinctive pedagogy in teaching jurisprudence; the accretion of explanatory material stemming from the medieval glossators and their successors that accompanied the *Corpus Iuris Civilis* provided a common body of texts and problems; and above all, indispensable to the law was that it prove workable in practice. Moreover, despite Justinian's famous prohibition of legal commentary—the *Corpus* was intended to be self-sufficient—interpretation inevitably became nec-

essary. Jurists themselves, although wary of the precarious theoretical status of interpretation, nonetheless felt compelled to articulate and to defend the modes and methods of interpretation they applied to legal texts. Issues of signification, definition, evidence, authority, conjecture, cavillation, the relation of the *mens legislatoris* to the *ratio legis*, and the roles of custom and equity all appear in Renaissance legal writings. It is on the steadily proliferating body of sixteenth-century commentaries on key jurisprudential texts devoted to interpretative rules and terminology, most notably Digest 50.16 *De verborum significatione*, that Maclean has focused his attention. Among the many virtues of this approach is to reveal how persistently such matters of interpretation and meaning were raised by Renaissance jurists.

What major conclusions does the investigation of these texts reveal? In the first place, despite the well-known controversies between schools of jurisprudence in the sixteenth century, particularly the opposition between the post-glossator inspired *mos italicus* and the more humanistically informed *mos gallicus*, Maclean is convinced that the legal profession showed more common ground than division. Reliance on medieval predecessors marked both traditions of legal studies and affected publishing practices, the characteristic mode of legal arguments, and pedagogy. Furthermore, continuity, not change, describes legal studies in the Renaissance to the point that the author suggests a "paradigm" existed from 1460 to 1630 on issues of interpretation. Second, the impact of humanism on legal studies is less consequential than previous scholarship has suggested. Humanism's more historically grounded philological examination of legal texts did not fundamentally alter the jurists' theories of language and of logic. Both medieval and Renaissance jurisprudence involved a "mentalistic" approach to language—that mental events precede any linguistic utterance—a notion ultimately grounded in a general Aristotelian intellectual context. Only in a greater emphasis on the "performative" function of legal language—that the words of the law create enforceable obligations or actionable consequences—did the more rhetorical elements of humanist thought have some impact. Yet this impact was limited, for legal theories did not find such notions incompatible with an emphasis on law as subject to dialectical rules of argument.

Third, although Renaissance legists were to some extent aware of the logical and epistemological limitations of their theory of language, especially with regard to such issues as the legislator's or the testator's intentions, no general crisis of language resulted (pace Richard Waswo's *Language and Meaning in the Renaissance* [1987]), in part because an otherwise logically shaky distinction between the *verba* and the *ratio* of the law could be usefully deployed in actual legal practice. Finally, Maclean suggests that a perennial "legal" view of language and interpretation can be traced from the Middle Ages to the present.

Where issues persist, as in the relation of legislative intent to the demands of equity, a common ground of conceptual problems remains. This being the case, we can in some measure transcend the radically relativist position that, being bound by our own conceptual paradigms, we are cut off from the past. An intellectual history more attentive to the disciplinary outlook and concerns of past thinkers therefore offers a more promising approach to grasping their intellectual concerns.

CHARLES L. STINGER
State University of New York,
Buffalo

R. C. VAN CAENEGEM. *An Historical Introduction to Private Law*. Translated by D. E. L. JOHNSTON. New York: Cambridge University Press. 1992. Pp. viii, 215.

The publisher's blurb to this brief book by one of the most eminent historians of European law states that R. C. van Caenegem "pays particular attention to the origin of the Common Law—civil law dichotomy" (p. iii). In the preface to the English-language edition, van Caenegem describes his effort as an "attempt to weave the historic threads of common and civil law into one fabric" (p. vii). With these thoughts in mind, the reader may well be surprised to read, at the beginning of the book, that it is intended "to give a historical introduction to the development of the private law currently in force in Belgium and the Netherlands" (p. 1).

This translation from *Introduction historique au droit privé* (1988) displays its origins as lectures given to the author's classes at the University of Ghent in several ways; German titles are translated while French ones are not and the book begins with a chapter devoted to the Napoleonic *Code civil* of 1804. In discussing the antecedents of the *Code civil*, however, van Caenegem expands his view far beyond northwestern Europe. A brief chapter on the early Middle Ages is followed by a long one on Europe and Roman-German law, ca. 1100–ca. 1750. This chapter persuasively argues against the traditional periodization that gives prominence to the sixteenth century. Chapters on the Enlightenment and the nineteenth century (which virtually ignore southern Europe) are followed by two concluding chapters that discuss in general terms sources and factors in legal history. The focus is on "external" history, for example individuals and texts, although substantive private law does emerge occasionally—curiously, most often in the discussion of canon law.

Scattered throughout the book are particular insights that are persuasive, including van Caenegem's comments on the preference for the spoken over the written word in Frankish capitularies or his comparison of the *mos gallicus* with Ciceronian Latin in its stultifying effect on Renaissance culture, and others

that are less so, such as his claims for the uniqueness in a global perspective of canon law. English law is dealt with in two ways. Occasionally he discusses it in and of itself, as in the section he devotes to Lord Mansfield, William Blackstone, and Jeremy Bentham. English law also pops up in the midst of discussion of developments on the Continent; within a discussion of the influence of Roman law on canon law is the reminder that "the Common Law shows exactly how a European post-feudal law might have developed in isolation from the Roman model" (p. 59). Those familiar with van Caenegem's work on Anglo-Norman law will not be surprised to see the English legal system characterized as "nothing other than a continental feudal law" (p. 178), while others may dispute the claim, made with reference to nineteenth-century England, that "it is a paradox that the most economically and socially advanced nation in the world had only a medieval legal system" (p. 160). More than a historical introduction to the contemporary law of Belgium and the Netherlands, less than a study that pays particular attention to the dichotomy between English and Continental law, van Caenegem's essay is consistently well informed (the bibliography is superb), occasionally provocative, and usually persuasive.

MICHAEL D. GORDON
Denison University

KATHLEEN WELLMAN. *La Mettrie: Medicine, Philosophy, and the Enlightenment*. Durham, N.C.: Duke University Press. 1992. Pp. xiv, 342. \$34.95.

This is the first comprehensive study of Julien La Mettrie in English. Kathleen Wellman illustrates how his thought, growing out of the concerns of a physician, was a uniquely effective example of the medicalization of the Enlightenment. While such an interpretation is not new, Wellman discusses more fully the medical background of her subject: La Mettrie's writings on medicine, his satires against doctors, his advocacy of Boerhaave's empirical and clinical teaching, and so on. While La Mettrie is thus recognized properly as a disciple of Boerhaavian iatromechanism, his skepticism about some mechanical explanations of disease and health is confused with what Wellman takes to be his rejection of a mechanistic physiology as such. But not a single supporting passage is quoted from La Mettrie's medical works.

Wellman's book has revisionist implications for the historian of ideas. In particular, she denies a linkage between René Descartes's *bête machine* and La Mettrie's *homme machine*. The two doctrines may be summarized as follows: although animals appear to feel and think, they are nothing but machines, therefore essentially distinct from human beings, who do feel and think (Descartes); if animals are machines that appear to feel and think, then human beings, who also appear to feel and think, are machines, therefore

not essentially distinct from animals (La Mettrie). The first proposition is admittedly different from the second, but it is easy to get from one to the other. The required "logic" is the quite ordinary reversal of a proposition within the terms of the argument framed by the proposition itself. La Mettrie himself averred that such an act of reversal was what Descartes's *bête machine* had inspired him to do. He affirmed this unambiguously in *L'homme machine* (1747) and in *Les animaux plus que machines* (1750). In the *Traité de l'âme* (1745), he remarked that he "could have" invoked the same reasoning. The title of *L'homme machine* plainly rhymes with *bête machine*. Moreover, a long tradition of commentary, both for and against animal automatism, had prepared the way historically to such an inversion of the Cartesian paradigm, before La Mettrie dared to do it publicly and resolutely. It is puzzling why Wellman resists taking La Mettrie's plausible word for it that Descartes's thesis on animal nature led him, by analogy, to his parallel thesis on human nature.

Wellman argues instead that La Mettrie's harsh attacks on Cartesian metaphysics prove that he could not have been influenced positively by the *bête machine*, as he asserts he was. Actually, the opposite is true. In order for La Mettrie to credit the materialist denouement he brought to the *bête machine* story, he logically had to demolish the barrier of Descartes's metaphysics, which is why he gave that task a high priority. His intentness on refuting Cartesian dualism, innate ideas, and the rest, confirms the continuity he alleged between the *bête machine* and his own *homme machine*.

Having dismissed La Mettrie's explicit claim of filiation, Wellman is at pains to present his *Traité de l'âme*, if not *L'homme machine*, as a French avatar of Lockean empiricism. But there is no significant reference to John Locke in the text of the *Traité de l'âme*, let alone any admission that its materialism derives from him. Locke is mentioned three times in passing: twice as having wisely limited himself to the investigation of "secondary causes," and once for his "solution" of the Molyneux problem (with which, incidentally, La Mettrie disagreed). The *Abrégé des systèmes*, a supplement to the *Traité*, contains brief appreciations of Locke and several other modern philosophers. It repeats La Mettrie's approval of the Lockean reluctance to pry into essences and first causes and announces his acceptance of sensationist epistemology. Locke is even called a herald of materialism, but only for his supposition about the possibility of "thinking matter," not his theory of knowledge. And in *L'homme machine*, the only important references to Locke, passed over in silence by Wellman, have the aim of distancing La Mettrie from him, especially as to the origins of materialism.

A comparison of the *Traité de l'âme* with Locke's *Essay Concerning Human Understanding* (1690) reveals that, whereas La Mettrie continually and typically describes mental events as the variable effects of

anatomical structures and physiological mechanisms, this whole dimension of a physical substratum of mind is absent in Locke. Although the two approaches—the physiological and the psychological—are complementary, as La Mettrie recognized, one neither is, nor stems from, the other. Nothing, for instance, in La Mettrie's *oeuvre* resembles the sort of meditation on Lockean psychology and epistemology that is found in Denis Diderot, Étienne Bonnot de Condillac, and Claude-Adrien Helvétius. Wellman cannot help but note the contrasts between La Mettrie's and Locke's method of explaining mental operations, but she fails, strangely, to see that these contrasts point to different schools of future psychology, and to different philosophical positions, which in La Mettrie's case was mechanical materialism, and in Locke's a conventional, if somewhat vague, separateness of body and mind. As the *Essay* puts it (IV,X,16): "unthinking Particles of Matter, however put together, can have nothing thereby added to them, but a new relation of Position, which 'tis impossible should give thought and knowledge to them." This kind of immaterialist judgment is precisely the antithesis of the basic intuition of La Mettrie's materialism. Concerning such a nonmechanistic representation of matter, he responded ironically: "demander si la Matière peut penser sans la considérer autrement qu'en elle-même, c'est demander si la Matière peut marquer les heures. On voit . . . que nous évitons cet écueil, où Mr. Locke a eu le malheur d'échouer" (*L'homme machine* [1960]).

A materialist philosophy must be founded on a theory of matter. La Mettrie's theory was that, if matter is organized in an appropriately complex system, the internal motions of its components produce life, consciousness, feelings, and intelligence. He defined such an interactive system as a "machine" and its movements as "mechanical," citing the clock, the best machine that eighteenth-century technology could offer, as a model for what he meant. His posterity today would no doubt use other models and terminology, but that is not the point. Wellman is loathe to make La Mettrie's materialism consist of anything more than his observations that certain bodily states are connected causally to certain psychic manifestations. Such objective knowledge is unquestionably part of an empirical science of medicine. But in itself it is neither philosophy nor materialism. If that is all La Mettrie had to say, those who, like Ernst Cassirer (in his *Philosophy of the Enlightenment* [1951]), are blamed by Wellman for minimizing La Mettrie's importance would be right not to have taken him seriously as a philosopher or an original thinker.

La Mettrie, in *L'homme machine* and elsewhere, consistently and insistently employs the terms and concepts of "machine" and "mechanism" to characterize the physiological basis of emotion and thinking. Wellman thereupon decides that his materialism is nonmechanical. But she does not tell us what "non-mechanical" properties of matter such a materialism

relies on. This is not surprising, because La Mettrie did not assume that any nonmechanical properties of matter participate in the causation of human or animal mentality. If the visual experience of a printed text may be considered empirical evidence, Wellman's reading of La Mettrie is not an exercise in the empiricism that she praises as his foremost intellectual virtue. It is fashionable nowadays for critics to discover that writers end up expressing the opposite of what they mean. But Wellman is not a deconstructionist in the usual sense. The hermeneutic art by means of which she develops her thesis may be described more aptly through a metaphor dear to La Mettrie himself, the "*ouvrage de Pénélope*": what is woven on one page is unwoven on the next.

It is obvious that Wellman dislikes mechanism as a notion applied to the mind or the organism. One wonders, then, what attracted her to La Mettrie. Her motive must have been to rehabilitate the *enfant terrible* of the Enlightenment by contending that, regardless of what he wrote, he was not really a mechanist. A materialist and atheist, to be sure; but not someone so perverse as to equate man with a machine. What, in the eighteenth century, was the scandal of La Mettrie still has the power to offend, repel, and repress in our own.

ARAM VARTANIAN
University of Virginia

RICK SZOSTAK. *The Role of Transportation in the Industrial Revolution: A Comparison of England and France*. Buffalo, N.Y.: McGill-Queen's University Press. 1991. Pp. xi, 331. \$44.95.

As Rick Szostak's title implies, this book is dedicated to the proposition that an efficient transportation system was essential to the Industrial Revolution. That the Industrial Revolution started in England is thus closely connected to the fact that eighteenth-century England had "the best transport system in Europe, and thus the world" (p. 48). France, by comparison, had an ill-organized system afflicted by local isolation, capricious royal projects, and erratic maintenance. Consequently French entrepreneurs and innovators were unable to create and to respond to economic opportunities as readily as did those of England.

Szostak sets out not to prove these propositions (which are hardly controversial), but rather to show that the various explanations of the economic development of France and England have understated the importance and pervasiveness of transportation as a necessary (if not sufficient) factor in economic growth and innovation. Because of the time frame used, the real topic is the sustained, self-reinforcing interaction between transportation and economic growth rather than the origins of that interactive process. Szostak begins with a theoretical discussion of the problem, ultimately offering an interactive flow chart that il-

lustrates the numerous and cumulative efficiencies created by better (and better integrated) means of transport. This is bolstered by a brief review of Anglo-American economic historians whose work touches on the problem at hand. Thus, we get brief summaries of work by Patrick O'Brien, Caglar Keyder, Richard Roehl, Nicholas Crafts, Joel Mokyr, Robert Fogel, and Douglass North, each showing the need for a more explicit discussion of transportation.

This is followed by a long chapter that provides a great deal of information about the transportation systems of England and France. The material on England is particularly interesting and represents a searching investigation in primary sources for data and descriptions. Szostak draws a remarkably vivid picture of the evolving transportation network in England, its growing complexity and integration, and its ability to increase the size of the national market available to any producer in the country who was ready to engage in extra-local economic activity. He also documents the capacity of the system to sustain rapidly increasing levels of regional and functional specialization. The sections on France are similar in approach, but more thinly documented. For that reason, and because of the much greater size of France, they seem more superficial. Although it is hard to quarrel with the comparative conclusions, the discussion of France is less precise and convincing than that of England.

The remainder of the book is devoted to selected industries (metallurgy, textiles, and pottery) in both countries, showing repeatedly how the different transportation environments contributed to the growing industrial gap between England and France. While Szostak regularly brings the discussion back to transportation, these chapters tend to read like catalogues of specific technical innovations in each of the trades examined. They are nevertheless extremely full compendia and offer a useful starting point for anyone seeking to enter into the technological history of the eighteenth century. These chapters also reiterate the image of growth as a widely diffused process that is offered by Maxine Berg, Myron Gutmann, and other scholars associated with the discussion about protoindustrialization in Europe.

For me the book suffers from two liabilities that weaken its effectiveness. The writing style is awkward and could have been helped with better editing. The text is hard to follow at times and often lapses into platitudinous statements that are less specific than the author intends. This is unfortunate because the topic and approach are certainly worthwhile. The second concern involves part of the analytical frame of reference. Although Szostak does briefly compare northeastern France with England, for the most part the comparison is between England and France as a whole. This is awkward, given that in France the regional structure of economic life did not correlate with the political boundaries of the monarchy as tidily as in England. Consequently, the comparison is often

between a single, well-defined economic region (England) and a cluster of distinct economic regions (France). Some French regions were actually fragments of larger regional systems that extended beyond the authority of the French crown.

As an analysis of English transport and economic development, this book is quite informative and convincing. As an analysis of the same process in France, the results are less satisfactory, a problem that inevitably also weakens the attempt at systematic comparison.

DAVID R. RINGROSE
University of California,
San Diego

S. H. RIGBY. *Engels and the Formation of Marxism: History, Dialectics and Revolution*. New York: Manchester University Press; distributed by St. Martin's. 1992. Pp. viii, 256. \$69.95.

This is a serious, interesting contribution to recent scholarship on Friedrich Engels. S. H. Rigby argues that Western Marxists in their studies of Karl Marx tend too often to scapegoat Engels, accusing the latter of grossly stereotyping Marx and making Marxism more rigid than the master intended. From such a standpoint, Engels rather than Marx was the dogmatic Marxist. Rigby instead stresses the close relationship between Marx and Engels throughout their intellectual careers. If there were changes over the decades, both were implicated.

In carefully researched accounts, interspersed with evaluations of other authors, Rigby proposes that Engels's intellectual outlook, in close partnership with Marx's, went through three stages. In the first period, prior to *The Holy Family* (1845), Engels wrote as a Hegelian socialist, viewing history as the teleological unfolding of the principle of Humanity. This would change with the new emphasis on class struggle and materialism in the later 1840s: "In the second, pragmatological period, including *The Condition of the Working Class in England* (1844), *The German Ideology* (1847), and *Communist Manifesto* (1848) and his writings on the Reformation and the German Revolution of 1848, Engels retained his anthropogenetic conception of the course of history but now expected a fully human society to be brought into being by the material interests and agency of the proletariat" (p. 236). Rigby argues that Engels never abandoned this view, although in the third phase he would tend to approach Marxism "increasingly as an objective science, a nomological outlook which had established the laws of social evolution" (pp. 236-37).

DONALD M. LOWE
San Francisco State University

JAMES A. WINDERS. *Gender, Theory, and the Canon*. Madison: University of Wisconsin Press. 1991. Pp. xi, 195. Cloth \$37.50, paper \$12.95.

One of the central concerns of the second wave of women's liberation was the struggle for control of discourse. Many women who had been active in the New Left were resentful of male domination of meetings that derived from rhetorical superiority: masculine discourse was abstract, intellectual, impersonal, and it served to exclude women who treated language as an intimate means of personal communication rather than as a weapon of ideological battle. Marge Piercy, in her influential essay "The Grand Coolie Damn" (1969), outlined how movement women sought to subvert male domination of discourse by their recognition that the personal is the political and by employing a language far more accessible than that of the "machers" who dominated the New Left. Therefore, it is ironic that James Winders, a self-proclaimed feminist, adopts that very opaque and abstract language that second-wave feminists abandoned because they viewed it as an instrument of patriarchal control.

Winders aims to provide a reinterpretation of five canonical texts in European intellectual history that takes into account issues of gender. He argues that these selected texts by René Descartes, Karl Marx, Gustave Flaubert, Sigmund Freud, and Friedrich Nietzsche either repress any discussion of gender or represent the feminine in a distorted fashion at key points in their discourse. It is difficult, however, to discern any originality in his claim that their writing is phallogocentric or in his own use of theory; in fact, Winders overtly proclaims that he has "no startling new theory of gender to propose" (p. 15) and instead is heavily reliant on the work of Teresa de Lauretis.

Although Winders's project in these essays is ostensibly a feminist and democratic one, he is attentive to the rhetorical nuances of the canonical texts he selects but oblivious to the political implications of his own work. The choice of esoteric discourse is by nature exclusionary whereas his own purpose is allegedly democratic, creating a new politics of teaching that dethrones the textbook by empowering students to confront actual texts and using feminist theory to deconstruct patriarchal canonical texts. Winders fails to explain adequately, however, the debates over the texts he selects, thereby choosing a style that privileges those already privy to the debates and disenfranchises those others unfamiliar with them. Winders also controls discourse in an arbitrary fashion by his whimsical allusions to popular culture that extract the excerpts from their original context to insert a new authorial meaning (concluding, for example, that his rereading of canonical texts means that there is a "whole lot of shakin' goin' on" [p. 142]). Jerry Lee Lewis has been accused of many crimes, but attacking the defenders of the canon was not among them.

Winders claims to be a postmodernist, to operate in a universe in which knowledge is fragmentary and elusive, yet his own stance is that of a Renaissance man able to comment with equal lucidity on all aspects of European intellectual history and modern

American popular culture as well. As a postmodernist he repudiates humanism as an archaic notion, yet he makes himself the hero of his own romance, filtering all his discussions, such as that of Descartes' *Meditations on First Philosophy* through his own experience as a teacher or researcher. Winders's penchant for writing to fellow travelers—"Freud, as is well known" (p. 94) or "I would have a recurring dream about visiting Freud's office, which, as everyone knows, was located in his home at 19 Berggasse in Vienna" (p. 95)—belies his stated intention of democratizing learning and converting his readers to a feminist, global, and interdisciplinary approach to culture. If one is writing to the already converted, why bother to preach?

LESLIE FISHBEIN
Rutgers University,
New Brunswick

AUREL SCHUBERT. *The Credit-Anstalt Crisis of 1931*. (Studies in Monetary and Financial History.) New York: Cambridge University Press. 1992. Pp. xiv, 205. \$54.95.

The financial instability of the past two decades and especially the 1980s led Aurel Schubert to investigate the collapse of the Austrian Credit-Anstalt (CA), one of the most dramatic bank failures in history. On May 8, 1931, in the depths of the Great Depression, officials of the bank, the largest in Central and Eastern Europe, revealed staggering, unanticipated losses amounting to 7.5 percent of its total assets and 85 percent of its equity. Three days of top-level secret meetings involving officials of the bank, the government, and the Austrian National Bank ended in a plan for reconstruction that bailed out the CA.

Schubert outlines how the crisis of this one bank precipitated a run on the Austrian banking system that was halted by the combined efforts of the central bank and the government, but in a way that launched a full-scale financial crisis and the devaluation of the Austrian schilling in 1934. The CA crisis not only deepened and lengthened the Great Depression in Austria; it was by most accounts a major factor in the world financial crisis that included the departure of Britain from the gold standard in September 1931.

Schubert's approach to this important episode in interwar economic history reflects his audience: largely economists with a historical bent, who turn to history as a testing ground for macroeconomic theory. This is clear not only from the motivating passages of the introductory and concluding chapters but also from the heart of the analysis in chapters 3, 5, and 6. Here the author places the CA collapse in the context of current theories of financial crisis. He uses data on CA share prices and exchange rates (including black market transactions in coffee houses) to test whether markets "predicted" either the CA collapse (they did so weakly) or the subsequent financial crisis (they did so clearly). He adopts a narrow

approach in analyzing the channels through which the crisis prolonged the Depression in Austria; the culprit was the subsequent drop in the stock of money that emanated (not surprisingly) from a disturbance on the supply rather than the demand side.

Historians of post-1919 Central and Eastern Europe and economic historians of the interwar era will find less of interest in these chapters than in chapters 4 and 7, which deal with the causes of the crisis and the response of policy makers to it. Although the dust jacket emphasizes "inconsistencies in policy" and the "reactions of unprepared monetary authorities," the author's discussion in chapters 4 and 7 points to a much richer and more complex interpretation of the CA crisis. In particular, the inadequate policy responses must be assessed in light of the structural changes following World War I. For example, a major source of the CA's insolvency in 1931 was the bank's decision to persist with its Danubian strategy after the dismemberment of the Habsburg empire rather than scaling down its operations to fit the small size of post-1919 Austria. Both the post-World War I hyperinflation and the terms imposed on Austria by the League of Nations that ended it shaped events in 1931; they fueled public mistrust and constrained policy options by the government and the central bank. The rather half-hearted efforts to deal with the crisis at the international level, by the Bank of England and the Bank for International Settlements, for example, reflect the more general breakdown in international cooperation that characterized the interwar years.

Schubert's discussion of the causes and consequences of the CA crisis is highly valuable but, as he himself admits, not the "definite story" (p. 31). This, according to the author, would require bank-level microeconomic data that are not now available, presumably to permit the same kind of formal hypothesis testing that he does in chapters 5 and 6. Having the right data would surely help, but it is not the whole answer. World War I both initiated and accelerated significant changes in fundamental economic institutions and policy regimes that stabilized only after 1945. For this reason, attaching "relative weights to the different contributing factors" (p. 31) behind the CA crisis requires a framework that goes beyond cliometrics and the theories of financial crisis the author uses to shape his analysis. Such a framework does not now exist in finished form, but its elements lie in the growing research by many economic historians on the interaction of the state and institutions in economic development. The motivation for using this body of work to rethink events like the CA crisis of 1931 lies in the recent collapse of the Soviet Union, which marks another watershed in the economic history of Central and Eastern Europe on the scale of World War I.

DAVID F. GOOD
*University of Minnesota,
Twin Cities*

ALAN KRAMER. *Die britische Demontagepolitik am Beispiel Hamburgs 1945–1950*. (Beiträge zur Geschichte Hamburgs, number 40.) Hamburg: Verein für Hamburgische Geschichte. 1991. Pp. 482.

The research of several recent economic historians has suggested that neither the wartime destruction of Germany nor the constraints attending the British and U.S. occupation of the country after World War II weakened the German economy as seriously as contemporaries believed. In this case study on British dismantling policies and practices in occupied postwar Germany, and particularly in Hamburg, Alan Kramer supports these views. Kramer's general conclusion on his important topic is quite striking. After defining "dismantling" as the destruction or removal of Germany's industrial plant by the Western Allies for purposes of demilitarization and reparations, he estimates that all dismantling reduced the gross fixed capital of West Germany in mid-1948 by only 3.51 percent.

This striking statistic, of course, is somewhat deceptive. Dismantling was but one of many restrictions on western Germany's postwar industrial performance. Among others, industry and labor suffered from losses caused by production limitations (as on the steel industry), from losses created by production prohibitions (as on shipbuilding), and from losses in plant investment due to business uncertainty and currency instability. Kramer is aware of these and other related and often unquantifiable macroeconomic variables. Indeed, after detailing the microeconomic process of dismantling in Hamburg, he concedes that its economic and sociopolitical consequences in the port city are difficult to determine. He does venture two notable statistics for Hamburg. First, dismantling and war damages reduced the city's shipbuilding capacity by 55 to 60 percent of prewar levels; second, dismantling reduced the capacity of Hamburg's machineries industry—the second hardest-hit industrial branch—by 14 percent (a figure that he deems somewhat high).

Although these losses do not seem inconsequential, they fail to deter Kramer from his conclusions. Dismantling, he asserts, contributed considerably to the economic recovery of the nineteen member-nations of the Inter-Allied Reparation Agency, but it did not lead to a considerable weakening of the western German economy. The latter broad assertion is not fully proven. To be sure, Kramer shows that the process of British dismantling in Hamburg was remarkably conflict-free and thus different from other cities in the British zone. But neither this conclusion nor others on the process of removing the industrial plant in a single city adequately supports the broad assertion about the small quantitative impact of dismantling on the larger western German economy.

As to Hamburg's economy, it is difficult to accept Kramer's statement that the city's lagging postwar recovery was not affected by losses from dismantling. The statistics cited above appear to contradict this

strong claim; moreover, he seems to overlook his own reference to such "unlisted" removals as 316 (evidently valuable) industrial machines taken from fifteen port city firms under an ingenious scheme called "multilateral deliveries."

Nevertheless, Kramer's case study does suggest that, for a host of reasons, Allied dismantling was not as catastrophic as western Germans either believed or as they claimed. He calls for further research to test whether German strategies of resistance to dismantling, which he models for Hamburg, were practiced elsewhere. This vexatious issue, along with others fruitfully raised, merits further exploration.

Kramer frequently focuses on economic matters, but he also offers much valuable information on the complex politics of dismantling. He probes the course and the fortunes of a British policy that essentially sought reasonable production levels for Germans and reasonable reparations for Britain. These British aims as well as dismantling became anachronistic when the Marshall Plan and the Cold War dictated a new policy toward the former enemy. For specialists, Kramer's challenging exploration of postwar Germany's economy furnishes many useful insights.

MANFRED J. ENSSLE
Colorado State University

NIGEL LLEWELLYN. *The Art of Death: Visual Culture in the English Death Ritual, c. 1500–c. 1800*. London: Reaktion; distributed by University of Washington Press, Seattle. 1991. Pp. 160. \$22.95.

The history of death has been "done to death" it might seem, in the light of a series of recent general historical studies, most notably that by Philippe Ariès, published in English as *The Hour of Our Death* (1981), which seems to have started a veritable academic mortification industry. But we have just begun to understand this most taboo terrain in our own culture which was, by contrast, an everyday experience for our ancestors.

The complexity and richness of the materials awaiting the historian of death is made evident in this admirable short study of the English arts of death from 1500–1800. Nigel Llewellyn has gathered a startling array of monuments and images in a variety of media that cumulatively represent early modern culture's attitude to that ubiquitous experience of death. In an example of the way history and art history can be combined in fruitful ways, using methods of both disciplines, the author examines amazing images like the Judd Marriage of 1560, whose nuptials take place over a rotting corpse. Most refreshing is the author's thematic treatment, which ranges from traditional allegories of death personified, in the dance and at the deathbed, to the civic rituals associated with internment and finally to church monuments. In a series of nineteen short chapters, death is explored as a process rather than as a single event, a

ritual whose proper practice was of concern to all members of society and in effect helped organize the life of the survivors.

The book was published in tandem with an exhibition held at London's Victoria and Albert Museum in 1991. This accounts for the brilliant variety of objects and paraphernalia that the author exhumes and examines. Death's-head spoons, fans, wax-effigies, and jewels, as well as the more traditional sepulchral arts are discussed in a lively text that always keeps the interest of both the general and the specialist reader. Llewellyn frames the book with a careful theoretical structure, laying out the anthropological as well as historical sources for understanding the subject, and he provides a useful bibliography.

Llewellyn's thoughtful epilogue makes a point that is repeated by all post-Ariès historians of death: that the fascination with death and decay celebrated with such gusto in the past starkly contrasts with the fear and erasure of the subject in modern life. But after reading this book and thinking of all the eager museum-goers who enjoyed the Victoria and Albert exhibition, I am struck rather by the current willingness to once again face the skull, that mysterious part-object that Sir Thomas Browne called "the true theory of death." With memorials like the AIDS quilt traveling the world to mass mourning and the recent rethinking of euthanasia laws, perhaps today death is not the abnormal event it became for the Victorians, but a final fantastic show, an art "happening." Death itself might be changing in response to our history of it, and this book's evocative display of its past provides some pointers toward its postmodern future.

MICHAEL CAMILLE
University of Chicago

MARJORIE KENISTON MCINTOSH. *A Community Transformed: The Manor and Liberty of Havering, 1500–1620*. (Cambridge Studies in Population, Economy and Society in Past Time, number 16.) New York: Cambridge University Press. 1991. Pp. xviii, 489. \$69.50.

While dancing his famous "nine days wonder" in 1599, William Kemp, the comic actor, took two days respite at Romford, the Essex market town that was the economic and administrative center of the royal manor and Liberty of Havering-atte-Bower. Kemp's choice of a stopping place on his way from London to Norwich tells us a good deal about Havering's socioeconomic history between 1500 and 1620. Laying near to the metropolis, athwart a main road leading from the capital into East Anglia, it was subject to virtually all the energies of socioeconomic, political, religious, and cultural transformation that concentrated in southeastern England in the Tudor and early Stuart age. Marjorie Keniston McIntosh's impressive study, a fitting sequel to her fine *Autonomy and Community: The Royal Manor of Havering, 1200–1500* (1986), documents the changes initiated and

experienced by the inhabitants of the royal manor as they faced these destabilizing developments and lost their "shared outlook of the later medieval years" (p. 1).

The book begins by showing how heavy immigration into Havering after 1500 disrupted old communal loyalties and how changes in agriculture, trade, and manufacture increased the social distance between the wealthiest and poorest inhabitants of the manor. Where community life had previously depended on the participation of a broad group of middling families, by 1600 power became concentrated among a landholding elite headed by a handful of noble and gentle families who competed with one another for honor and reward. In 1607, a divisive election in the Liberty brought these leading families to loggerheads and left lasting scars. Religious and cultural differences also undermined the capacity of Havering's inhabitants to maintain social cohesion across class and occupational lines. When challenges to the autonomy of the Liberty and the customary rights of its tenants came from the crown, as it did beginning in 1608, the community lacked the moral and political resources to unite in collective defense.

McIntosh sees these conditions as calling forth a systematic program of social control from Havering's leaders. In making her case, she challenges the existence of any "necessary link between social control and Puritanism" (p. 250). Instead, she views the regulation of social behavior as springing from more practical concerns about the disruptive social consequences of disorderly conduct and the dire effects on parish rates of sexual impropriety. Similar behavioral regulations appeared, she argues, in the later fifteenth century when local officials, desiring to maintain order in the face of rapid change, also sought to suppress social misbehavior, especially among the poor. Hence, for her, Puritanism offered little more than "a convenient language of expression and a reassuring spiritual justification for attitudes which probably would have developed anyway" (p. 251). No doubt McIntosh is correct in emphasizing pragmatic aims and consequences, but her account risks reducing the meaning of cultural forms to their social functions. It seems clear that there were many godly Protestants in Havering whose religious convictions conditioned their identities and shaped their relationships within the community and to the larger world. For them, the language of Puritanism appears to have provided more than a source of reassurance for the pursuit of secular interest.

In treating the breakdown of communal cooperation in Havering during the early modern period, McIntosh stresses a further change from its medieval past, namely its "integration into a broader world" (p. 405). She gives special emphasis not only to the manor's connections with London and the wider economic environment to which it gave access by 1500 but also to the ways in which the education of the elite in the sixteenth century linked its members

to their counterparts elsewhere in England, and to how the loss of the manor's ancient privileges "brought expanded involvement in a wider political context" and a growing awareness of membership in "a politically conscious nation" (pp. 405–06). In some sense, William Kemp's nine-days morris dance offers a fitting metaphor for these developments since it symbolizes, in the comic topsy-turvy of market-oriented self-promotion, the existence in England of newly emerging sociocultural forms, and marks London's dominant place in spreading them. Havering's history in this period seems very much the history of a local community caught up in the vortex of large-scale processes of urbanization and modernization centered on the metropolis. Many of the individual points that McIntosh describes are explicable as elements in this larger story. From this perspective, Havering's integration into the wider world is not just another kind of change of equal weight with the rest, as McIntosh interprets it, but the dynamo that moved its inhabitants from the spirit of shared values and the inward-looking practices of social cooperation so evident in the later Middle Ages, to the much more diverse views, the more deeply stratified social structure, and the self-regarding focus on worldly advantage characteristic of Havering's life in early modern times.

McIntosh's richly documented book provides important insights into the effects of social change on a community caught in processes it did not originate and could not control. Although in telling her story she might have paid somewhat greater attention to these pressures from without and done somewhat more to analyze how they drove the various developments she treats, she has given us a laudable account of life in a small community as it made its transition from the medieval to the early modern age.

DAVID HARRIS SACKS
Reed College

RICHARD HELGERSON. *Forms of Nationhood: The Elizabethan Writing of England*. Chicago: University of Chicago Press. 1992. Pp. xi, 367. \$27.50.

Richard Helgerson's book is an original and important contribution to English social and intellectual history, to a better understanding of "history and literature" as a separate field, and to a paradigm interpretation of English literature and historiography in the period from about the 1580s to the 1640s. "Never before or since," Helgerson observes, "have so many works of such magnitude and such long-lasting effect been devoted to England by the members of a single generation" (p. 1). In very different ways Edmund Spenser, Edward Coke, William Camden, John Speed, Michael Drayton, Richard Hakluyt, William Shakespeare, and Richard Hooker, along with others, created the idea of England, "the articulation of England itself" (p. 152). All took England, "its

land, its people, its institutions, and its history—as their subject” (p. 1).

The book looks back to pre-Elizabethan and Continental attitudes and forward to “the making of Britannia” in the eighteenth and nineteenth centuries. Helgerson begins by teasing out the implications of Spenser’s phrase: “Why in God’s name, may not we, as else the Greeks, have the kingdom of our own language?” The ambition to recast the language (especially of poetry) was part of a much broader ambition to create new “forms of nationhood.” Each of the six chapters might be read as a separate essay, but the book’s complex argument only emerges from reading the whole. The poets, lawyers, chorographers, propagandists for overseas expansion, playwrights, and churchmen belonged to “different discursive communities” and “wrote England differently” (p. 5), yet each showed unexpected similarities with the others, and each was linked to the broader idea of the Elizabethan writing of England.

Helgerson provides signposts that indicate the general direction of his argument: “For these Elizabethans, the way to an acceptable national self led through self-alienation. They had to know themselves as the barbarous or inferior other . . . before they could undertake the project of national self-making. In this sense, to be English was to be other—both before their work began and after it had been accomplished” (p. 243). There are surprises and ironies in the argument: Hooker, for example, asserted a God-given order, yet in the end argued that “bishops and kings owe their position not to heavenly appointment but to human law, to an act, that is, of the body politic” (p. 281).

Helgerson is fully aware of polarities and contradictions; he appropriately qualifies his arguments about unity and diversity. His book is a masterly survey, a learned and original interpretation, and a beacon for historians entering unfamiliar waters.

F. SMITH FUSSNER
Reed College

WALLACE T. MACCAFFREY. *Elizabeth I: War and Politics, 1588–1603*. Princeton: Princeton University Press. 1992. Pp. xv, 592. \$65.00.

The final volume in Wallace T. MacCaffrey’s trilogy on Elizabethan government and policy, this is a history of the concerns of the monarch from the Armada, “the definitive turning point of the Queen’s reign” (p. 3), to the end of her life. The most important of those concerns, all “short-term in nature” (p. xi), was the war with Spain, viewed here only from London’s admittedly ill-informed perspective. Also looming large in the story, however, are the accelerating Irish problem and new factionalism on the domestic front as an earlier generation of councilors died off. This is unabashedly top-down history, but a model of its kind, meticulously researched in

state papers and carefully presented in great if sometimes burdensome detail.

MacCaffrey begins his account with a description of the structure and function of the council and its transformation from a quasi-judicial and executive body into an institution for the management of war on an unprecedented scale. The council was remarkably involved in details like victualing troops and dispatching powder to the counties, as well as in ordering conscription and deployment of forces and disposition of ships, and in receiving what foreign intelligence was available, all the while continuing its traditional hearing of private suits and dealing with domestic administration. That the war was ill-managed was a function in part of the inevitable inefficiency of the council, coupled, however, with other problems. The evidence indicates that “the government could mobilize potential military and naval might [as in 1588], but it could not formulate effective strategic plans, much less provide the chain of command, effective communication, reliable intelligence, or adequate supply” (p. 38) needed for efficient prosecution of the war, whether at sea, on the Spanish coast, in France, or in Ireland. This theme is illustrated again and again in specific accounts of attempts at naval assault and troop deployment on land. As if these problems were not enough, corruption and abuse abounded, and desertion by soldiers and officers was rampant.

Beyond the sheer inefficiency of the council, Elizabeth’s conduct of the war was troubled by serious divisions among her policy advisors. They fell into two camps: the ideological and activist “maximalists,” who demanded an offensive policy against Spain on all fronts, and the pragmatic “minimalists,” whose policy was purely defensive. Complicating any policy initiative was the self-interest of private investors in “enterprises” against Spain. It is no wonder that both policy and strategy turned out to be highly inconsistent. In France, an additional problem for Elizabeth was the unreliable behavior of her allies. Bitter wrangling among the queen, Henry of Navarre, and Essex is a constant refrain during England’s intervention in France after the assassination of Henry III. And in Ireland, the complexities of Gaelic clan warfare and Old English rivalries, conflict among members of the Dublin council, and competing patronage ties to London made consistent policy a virtually unrealizable goal (p. 361). Decisions taken in London were necessarily reactive and pragmatic.

Given the conflicting counsel Elizabeth received, the divergent goals of warring parties on all fronts, and the general inefficiency of central and imperial administration, it seems not quite fair for the author so frequently to refer to her “indecisiveness” (p. 116) and “timidities” (p. 131) in conducting war. The Cadiz expedition is a good illustration of the fact that financial and technological handicaps, along with disagreements and bad decisions by commanders in the field, were far more at fault than irresolution on

the part of Elizabeth. Surely, moreover, the queen's fear of sending her fleet off to harass Spain without adequate naval defense of the channel, given her awareness of Spanish invasion plans, was prudent rather than timid.

The queen is also condemned rather often for her "parsimony," but this, too, seems unreasonable in light of the evidence. The failed Breton and Norman campaigns, for instance, had less to do with money than with poor communication and the divergent aims of the English and French monarchs (Elizabeth wanting only to clear the channel of the Spanish threat, while Henry's eye was constantly cast toward Paris). In France, Elizabeth threw quite enough good money (and troops) after bad. She ought not be condemned for frugality.

As a study of the conduct of war in the sixteenth century, and of the complexities of Irish administration, this book is first-rate. To the extent that it tells us much more than we knew before about what Elizabeth was up against, it broadens our perspective on the queen and the central government. Like the two volumes that preceded it, however, it lacks attention to the localities as influences on policy. In spite of MacCaffrey's previous work as a local historian, he gives short shrift to the role of borough and shire elites, either at home or as frequently vocal members of Parliament. As an analysis of Elizabeth's role in policy making, moreover, the book seems not quite to do justice to a monarch also extraordinarily troubled in the 1590s by domestic social, economic, and religious problems. One need not make Elizabeth or her advisors saints to give them more credit than MacCaffrey does.

MARGO TODD
Vanderbilt University

DAVID UNDERDOWN. *Fire from Heaven: Life in an English Town in the Seventeenth Century*. New Haven: Yale University Press. 1992. Pp. xii, 308. \$28.50.

In August 1613 Dorchester was devastated by fire. Although the town was rebuilt within a couple of years, this traumatic experience made an indelible impression on the collective memory of the inhabitants, who, persuaded that God had punished them for their sins with a "fire from heaven," undertook a total reformation of their community under the leadership of John White. For the space of two generations, Dorchester became "the most 'Puritan' town in England" (p. 5). Although there had been no lack of preaching in late-Elizabethan Dorchester, the town's Protestantism appears to have grown out of conformity rather than conviction. Whether any of Dorchester's governors actually underwent conversion as a result of the Great Fire of 1613 is not revealed, but by 1623 fifteen of the eighteen capital burgesses who formed the corporation were reformers.

When David Underdown states that Dorchester was the most Puritan town in the realm, he does not mean that most of the inhabitants were converted to a more godly way of life; rather, their lives came to be highly regulated through a culture of surveillance involving not only strict law enforcement by sergeants, beables, constables, watchmen, churchwardens, and sidemen, but also a widespread taste for peeping through keyholes and cracks in walls. Some practices, such as churchales, plays, maypoles, and skimmingtons, were successfully eliminated, but with its many alehouses—both licensed and unlicensed—Dorchester remained awash in a sea of drink. Indeed, not knowing where to keep the money collected for charitable uses, the godly governors of Dorchester invested it in a very successful municipal brewery, which yielded a profit of £200–300 annually for Puritan philanthropies. Considering the fact that the principal cause of disorder in Dorchester was drunkenness and the unlawful sale of drink, one wonders what the godly governors were thinking of. They did manage to compel virtually all adults to come to church, if the amount of communion wine consumed is an accurate guide, but it was quite another thing to keep them awake and attentive during sermons. After the Restoration, which was welcomed by the capital burgesses and clergy, who were good presbyterians, the old festive culture began to reassert itself and the godly reformation became moribund.

During the time of godly rule, Dorchester built up a highly developed and generous system of poor relief and philanthropy. Underdown admits that it is difficult to say how much of this came in response to John White's inspired leadership and how much it reflected trends in poor relief observable in other English towns. Compelling evidence of Puritan charitable endeavors comes from parish collections when White and his assistants exhorted their congregations to contribute to the new hospital, for defence of the Palatinate, or in relief of the victims of the Irish Rebellion. The author uses the rich parochial and municipal records effectively, and the evidence from Dorchester, compared with the more meager documentation from other West Country towns, leads one to infer that Dorchester was extremely generous in terms of private philanthropy (at least toward those whom the godly thought were deserving). As Underdown again admits, the reformers came entirely from the town elite, and it is difficult to distinguish between the desire to bend the idle to labor, found in most English communities at this time, and the determination to do the work of the Lord. This ambiguity is as much a reflection of Puritan ideology as it is of inconclusive evidence.

Underdown has produced a remarkably vivid and readable account of the mental world of a Puritan community that will surely become a classic. The governing elite were aware of their own interests; they were well informed about, and had definite opinions concerning, the Protestant cause elsewhere

in Europe. By the 1620s, Dorchester had freed itself from the gentry and court influences that had dominated Parliamentary elections in the previous reign and would do so again following the Glorious Revolution. Underdown does not hold with the new orthodoxy of degrading the English revolution to the status of a "crisis." Indeed, he argues that Dorchester's Puritan governors were beginning to engage in a political discourse that had become "confrontational" in nature (p. 189).

ROGER B. MANNING
Cleveland State University

MARIANNE HESTER. *Lewd Women and Wicked Witches: A Study of the Dynamics of Male Domination*. New York: Routledge. 1992. Pp. 239. Cloth \$68.50, paper \$15.95.

This study of early modern witchcraft by Marianne Hester unites the discipline of historical research with that of the feminist theoretician. The first half of the work is an exposition of Hester's "revolutionary feminist approach," while the second, quite separate half explores the persecution of witches in sixteenth and seventeenth-century England.

Hester views the basis of male domination over women as the "eroticization of submission and dominance"; that is, acts of submission and dominance have been internalized as erotic by women and men respectively. Thus, "heterosexuality must be recognized as a political institution whereby male sexuality . . . serves as social control of women in the interests of men" (p. 77). Because desire has become artificially linked to unequal power relations throughout Western history, the institutionalization of those unequal relations has gone unchallenged until today.

Within this framework Hester presents a succinct review of the theoretical feminist landscape, positioning herself in opposition to bourgeois feminism (which seeks to work within the framework of a capitalist economy), radical feminism (which emphasizes women's experience and women's culture rather than analyzing the construction of masculinity), post-structuralist feminism (for "vagueness and lack of an agenda"), and socialist/feminism (which views women's oppression as linked to more fundamental class relations). The discussion of her own "revolutionary feminist" approach focuses not only on pornography and other forms of overt sexual violence against women but also on more benign male/female relationships as the primary means of shoring up gender inequality in all areas of life. Particularly interesting is her discussion of heterosexual domination from the viewpoint of antisexist men of the East London Men's Centre and elsewhere, who record their personal attempts to objectify women through sexuality. The discussion highlights both what is unusual in Hester's work and what is limiting. For example, in recounting the experience of an anonymous member of the Men's Centre, she criticizes his frustration with fem-

inist women who still seek sex with aggressive men, but she refuses to acknowledge that the man's frustration might be anything more than an attempt to dominate his female partner. For these men, and for the gay men whom Hester interviews, sexuality is primarily about power, not about men's own struggle for enhanced physical relationships.

The second section of the book, an analysis of witchcraft in England in the sixteenth and seventeenth centuries, is partially intended to counter criticisms of her own and other feminist theories as fundamentally ahistorical. More importantly, the section on witchcraft is intended to demonstrate a widespread attempt by men to maintain control over women during a period of religious, social, and political instability.

Hester introduces her analysis of the witch craze by presenting an overview of published literature on the subject by Alan MacFarlane, Christina Lerner, Carol Karlsen, and others. She distinguishes between analyses that view witch hunting as sex-related (arguing that the persecution of women is an outgrowth of anxieties about other, presumably more significant, issues), and sex-specific. This overview of witchcraft literature is a useful introduction to the subject. Hester makes no attempt, however, to distinguish between the in-depth research of Lerner or Karlsen and the more cursory analyses of Mary Daly or Barbara Ehrenreich and Dierdre English. She does not attempt to link the English craze to events on the Continent, in America, or in non-Western cultures. More important, her focus on sexual violence against women leads her to downplay or ignore the possible linkage between the witch craze and the European state system, or the possible increase in male anxiety about masculinity during a period of social restructuring. Moreover, there is no attempt to analyze the mentality of women actually accused of witchcraft, either by citing interrogations or by linking witchcraft to domestic crime, the ideology of motherhood and childbirth, or the practice of healing and white magic by ordinary women. Hester does offer readers an accessible account of the trials in England, particularly Essex, and she does deviate from MacFarlane's work in directing the reader's attention to the specifically sexual aspect of the accusations (for example, the importance of familiars who were sexually intimate with the accused women). Overall, however, the main contribution of Hester's study is as a spur to further in-depth research on the psychology of witches and their persecutors.

PHYLLIS MACK
Rutgers University,
New Brunswick

JULIAN MARTIN. *Francis Bacon, the State, and the Reform of Natural Philosophy*. New York: Cambridge University Press. 1992. Pp. xiii, 236. \$49.95.

This important study attempts to provide a contextual analysis of the origin and character of Francis Bacon's natural philosophy. Julian Martin argues that Bacon's natural philosophy should be understood as a subordinate part of a much larger reform program appropriate to a centralizing monarchy. Martin also hopes to rectify Bacon's declining reputation among historians of science. Rather than being a marginal figure, he argues, Bacon provided a persuasive model of how research into the natural world could be organized and conducted.

Chapters 1 and 2 show how Bacon came to believe that the problems of knowledge were concerns for statesmen. Martin also describes the political tradition in which Bacon was reared and to which he became committed. Chapter 3 focuses on how, in the 1590s, Bacon came to believe that natural philosophy could be transformed into an important support for the crown. There follows an exceptional chapter describing the state of English law and the legal profession. Even those without a special interest in Bacon will find it illuminating. Chapter 5, focused on *The New Atlantis* (1627), deals with Bacon's ambitions to reform the apparatus of governance necessary to create an imperial monarchy. For Martin it is apparent that Colbert's vision of a royal scientific institution comes closest to Bacon's vision of a natural philosophy appropriate to a centralizing imperial state. The book concludes with an examination of Bacon's vision of a reformed natural philosophy: its aims, its bureaucratic methods, and its procedures of discovery.

The importance of the law for Bacon's natural philosophy has been explored by others (Paul H. Kocher, Kenneth Carswell, Harvey Wheeler, and Mark Neustadt), but Martin is the first to link it with a larger program of political reform. Although the effort to connect Bacon's new philosophy with legal learning is quite persuasive, Martin underestimates the humanist element in Bacon's reform program. Bacon, like the humanists, attacked scholasticism and emphasized useful knowledge. And they, like Bacon, strove to use knowledge to improve the quality of human life by harnessing knowledge to the state. Many humanists became government servants and many government servants entered state service with the aim of implementing humanist values. Bacon's participation in court masques and his involvement with the Philip Sidney circle, both discussed by Martin, also suggest Bacon's humanist affinities, as does his association with learned statesmen and patrons such as Baron Burleigh, Francis Walsingham, and the earls of Leicester and Essex.

Although the political context of humanist thought has been well explored, Martin has made an important contribution in investigating the political context of early modern natural philosophy, and he has drawn attention to the still-neglected question of what the "new philosophy" owed to the legal tradition. If one cannot fully agree with Martin's argument, one must applaud a skillful effort to integrate political

and cultural history. While it is not difficult to characterize his general argument, it is more difficult to convey the book's erudition and richness of detail. Martin has made an important contribution to reassembling the separate pieces of Bacon the politician, the natural philosopher, and the literary stylist.

BARBARA SHAPIRO
University of California,
Berkeley

JACOB M. PRICE. *Perry of London: A Family and a Firm on the Seaborne Frontier, 1615–1753*. (Harvard Historical Studies, number 111.) Cambridge: Harvard University Press. 1992. Pp. xi, 191. \$30.00.

Jacob M. Price's distinguished work as an economic historian provides the context for his insightful comments on the tobacco trade by way of a biography of the House of Perry. Price ingeniously uses a range of court, bank, and public documents to correct misperceptions derived from Elizabeth M. Donnan's classic study of the Perrys ("Eighteenth-Century Merchants: Micajah Perry," *Journal of Economic and Business History* [1931], 70–98). Consignment was not the firm's predominant method of acquiring tobaccos during its best years under Micajah Perry I (1641–1721), and its subsequent importance was one symptom of decline under his grandson Micajah III (1695–1753).

Although telling a family story, Price knows "that for many professional historians, even one page of genealogy may be a page too much" (p. x), and therefore he confines most of this information to appendixes. As he did in an earlier study of the Russell-Lee-Clerk family empire, Price performs extended family reconstitution as an element of economic history. Rather than contributing capital, the wandering ancestors provided potential connections and associations with various communities in England, Ireland, Scotland, Massachusetts, Virginia, Antigua, and Barbados. Marriages of siblings did little for Micajah Perry I's business, and the favorable marriages of descendants dispersed resources without cementing central business links. Family appears to have made more withdrawals than deposits to the firm's prosperity.

Micajah Perry I's success in the tobacco trade was primarily his own achievement, at a time when hundreds of fellow traders were leaving an increasingly difficult business. Perry and Lane grew by reinvesting profits that might have approximated £5,000 a year in the 1690s. For thirty years the Perrys were by far the largest importer of tobacco in Britain. They continued to spread shipping risks by owning and freighting only very small portions of numerous ships. They bought tobacco through stores and salaried factors in the colony, as well as by consignment. They bought carefully, and in volume, from myriad English suppliers. Trade in indentured servants from Britain and Ireland was a sporadic ancillary business;

trading in slaves was much less frequent. Success came from the occasional wise decision or bit of good fortune, supplementing the unending concern for the thousands of details of what became a highly complex business.

The fall of the House of Perry had many causes, and Price weighs them with his legendary judiciousness. Structural changes in tobacco prices and supply, as well as Scottish competition, transformed conditions after 1725 in ways that destroyed most London tobacco firms in the next generation. Micajah Perry III's political preoccupations, as alderman of London and member of Parliament, distracted him from business and helped alienate some prominent planters. He simplified business by closing stores, dismissing agents, and buying ships that soon operated unprofitably. Ultimately his inheritance from his grandfather had not included industry or business acumen.

Price's meticulous work is welcome as the definitive biography of this merchant house, as an unexpected evocation of Price's wide-ranging career of scholarship, and as an inspiration to all those drawn to significant subjects that seem to lack adequate surviving documentation.

IAN K. STEELE
University of Western Ontario

J. D. DAVIES. *Gentlemen and Tarpaulins: The Officers and Men of the Restoration Navy*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1991. Pp. xii, 270. \$69.00.

For fifty years research into the Restoration navy as an institution remained substantially frozen; it was presumed that J. R. Tanner had done all there was to do. Tanner was heavily preoccupied with the legacy of Samuel Pepys. In fact, until recently all writings on the Restoration navy mirrored Pepys's opinions and tended to accept uncritically his account of the progress of administrative improvement, full credit for which the great diarist was not hesitant to claim. Now, within the last decade, we have the fresh approaches of Sari Hornstein, Peter Le Fevre, and J. D. Davies. Davies's book is a richly documented study of the sea-service personnel.

The first half of the book, topically organized, is somewhat disappointing. One reason is the author's difficulty in finding much to say that is cogent—or even new—about the men of the lower decks. The other reason relates to the issue highlighted by the book's title: gentlemen versus tarpaulins. The delineation of this issue in social terms, especially the statistical work showing when the great influx of gentlemen actually occurred (not until after 1667), is important and original, but the chapter aimed at addressing how the issue was resolved, "The Reform of the Officer Corps," is diffuse and inadequate. This is a pity, because the resolution of this issue may well

have been the most important occurrence in the long institutional history of the Royal Navy.

According to Davies, Pepys's "almost obsessional antagonism" (p. 36) to the gentlemen officers blinded him to the reality that many of them were brave and competent, while at the same time his administrative temperament inclined him to complain too much of their absence from duty and general unruliness. These criticisms determine the line of approach, and the effect is to rule out the possibility that Pepys's anxieties—admittedly expressed too vehemently, as is usually the case with private jottings—may have been basically reasonable. Although Pepys realized that it was not good for the navy's officers to remain narrowly educated and socially obtuse, he regarded the contempt which some of the gentlemen officers expressed toward the tarpaulins as ominous. Moreover, the English ruling class had traditionally shown very little concern for sea matters. Pepys regarded the high standard of professionalism achieved during the Interregnum as an accidental stroke of national good fortune that could easily be washed away by carelessness, ignorance, and politics, even though, as he acknowledged, Charles II's interest in the navy far exceeded that of any previous English monarch.

The threat seemed very real (not only to Pepys but also to his hard-headed and sensible associate, Sir William Coventry). The need in 1677 to introduce a minimum sea-service requirement and an examination on seamanship and navigation for entering lieutenants appears to confirm their anxieties. (Unfortunately, Davies does not investigate whether these requirements had the immediate impact Pepys claimed for them or whether the sea-service requirement was steadfastly enforced.) Was Pepys wrong to complain about the tendency of gentlemen officers to shirk their administrative duties? A ship preparing for sea needed its commanding officer in attendance; he was a crucial link in the administrative chain. While one must agree with Davies that the professional corps was built as much by the sea-officers themselves as any set of administrators—and it is certainly time to stop crediting Pepys for everything good that happened to the navy in this period—the emerging and severely faction-ridden corps needed direction and control from central authority. Until the Lord High Admiral's office developed a formidable secretariat, the task could not be well executed, but Pepys's sense of its importance was correct.

The second half of the book is a political history of the sea-officers during the Restoration period. It is a pioneering study (based in some measure on the author's cogent articles), expertly presented. Davies offers a concise, readable, and highly convincing narrative that skillfully traces the officer corps through the turbulent reigns and pays particular attention to its roles in 1660 and 1688. The result is a major contribution not only to the history of the naval profession but also to the history of Restoration

England generally. No historian of the period should fail to read it.

DANIEL A. BAUGH
Cornell University

RICHARD T. VANN and DAVID EVERSLEY. *Friends in Life and Death: The British and Irish Quakers in the Demographic Transition, 1650–1900*. (Cambridge Studies in Population, Economy and Society in Past Time, number 17.) New York: Cambridge University Press. 1992. Pp. xix, 281. \$49.50.

According to Richard T. Vann and David Eversley, this volume serves two constituencies: historical demographers and historians of Quakerism. Finding myself in neither camp, I would suggest a third: social and economic historians who have bought into the notion of the “virtue of counting,” and therefore value the gathering and assimilation of demographic events so skillfully undertaken here. The primary audiences to which the book is geared seem well served, but whether this exercise in historical demography is considered palatable to those who only dabble in the trade (and therefore may bristle at the prospect of digesting the seventy-odd maps, graphs, and tables) is a more vexing question.

In assembling their sample of English and Irish Quakers, circa 1650 to 1900, to whom to apply the technique of family reconstitution, so crucial to the observation of a variety of measures such as mean age at first marriage, marital fertility, and age-specific mortality, Vann and Eversley correctly choose to rely on the most informative records, registers of vital events that appear to have been transcribed with painstaking accuracy, family genealogies, and, for Ireland, family lists. Their initial concern seems to be to justify the selection of the Quakers as a group whose demography should be studied. Isolating the Quakers, demographic behavior appears problematic for a number of reasons. In the first place, as the authors note, entrance and exit from membership in the Society of Friends was rather frequent; thus, as a group, it was more open than other elites whose demographic behavior has been studied, such as Hollingsworth’s English peerage or Henry’s Genevan bourgeoisie. Second, the social and economic heterogeneity of the Quakers remains dubious. Although historians generally place them in the middle rank, such a characterization is tantamount to no characterization at all because of the wide latitude between the upper and lower reaches of that class.

Understanding the economic and social characteristics of this variable “Quaker” is crucial to the interpretive enterprise, because Vann and Eversley wish to contrast their demographic experience with the findings presented by E. A. Wrigley and R. S. Schofield in *Population History of England, 1541–1841: A Reconstruction* (1981) and subsequent papers. Without defining the economic and social milieu of the Quakers,

comparison between the data sets is problematic. If the demographic behavior of Quakers tracks that of the rest of early modern English society as observed by Wrigley and Schofield (as it does in fertility rates prior to 1750 [p. 134]), does it mean that being a Quaker mattered little to a woman’s fertility? Yet if we find certain differences in these measures (for example, rising mean age at first marriage in the second half of the eighteenth century and increased celibacy rates [pp. 90, 108]), can they be attributed to Quakerism?

Contrasts between their data and those of Wrigley and Schofield are more frequently discerned. But attributing differential behavior to a cluster of values considered “Quaker” may prove to be a doubtful proposition. Vann and Eversley’s comparisons between English and Irish Quakers (with a dash of the American thrown in) suggest that English Quakers frequently behaved more like English non-Quakers (in, for example, seasonality of marriage [p. 84]) and Irish Quakers behaved more like Irish non-Quakers (in, for example, mean age at first marriage and marital fertility [pp. 90–104]) than English Quakers behaved like Irish Quakers. Vann and Eversley are well aware of the dilemma, and they report their findings with admirable refinement, qualification, and caution.

LLOYD BONFIELD
Tulane University

DONALD E. GINTER. *A Measure of Wealth: The English Land Tax in Historical Analysis*. Buffalo, N.Y.: McGill-Queen’s University Press. 1992. Pp. xxvi, 711. \$75.00.

Donald E. Ginter has written a dense yet lucid and illuminating study of the utility for historians of the English land tax and its place in the historiographies of diverse fields of social and political history. Established in the 1690s, the land tax assessments have survived in annual series for all parishes and townships of England during the period 1780 to 1832; they provide details on ownership, occupation of land and buildings, types of property, and amounts of tax assessed. Because of these records’ abundance and richness, historians concerned with the impact of enclosure, the shape of rural social structure, or electoral behavior under the unreformed Parliament have relied heavily on them. Ginter believes that these historians have poorly understood the pitfalls of using the assessments, and therefore devotes much attention to a close explication of such difficulties; he finds that almost all studies that have made serious use of them are “fatally” flawed.

Those who have attempted to convert tax values to acreage equivalents have consistently erred. Those who have aggregated across parish boundaries, in studying rural social structure, should not have done so; nor, alternatively, should those who have constructed class intervals within townships have at-

tempted such an exercise. Historians have failed to consider the effects of variations in soil types on valuations, the impact of Catholic double-assessments on county quotas, and the extent of underrecording of smallholders from strictly rural areas in the land tax duplicates.

Many of these criticisms carry conviction but in places appear overstated, since they frequently depend on the degree of error a scholar may judge permissible; the particular method of analysis found wanting may thus be more open to interpretation than Ginter intimates. Indeed, the author does not appear to allow for any margin of error, and so seems to have erected too purist a standard. Ginter advises that studies employing the land tax should confine themselves to restricted periods, preferably to those in which complementary data can also be found, such as the rental valuations for property tax returned to the Board of Trade in 1814. A more reliable estimate of land tax burden can then be made by constructing proper ratios of rent-to-tax.

Ginter proceeds to undertake this task for Yorkshire in 1814, and then for the country as a whole in 1815, with ambiguous results. He expected to explain regional variation of tax burdens within counties by different rates of agricultural or industrial improvement (lighter tax burdens should have fallen on the more developed properties). But he found no such relationship for either the North or East Ridings of Yorkshire, nor for the industrial West Riding, in the distributions of his rent-to-tax ratios, and he shows a similar absence of correlations for the country as a whole. Nevertheless, he does find such a pattern when the variables composing his ratio are run separately. This discovery suggests that his rent-to-tax ratio is itself an inadequate measure, and that the land tax values alone probably possess greater viability than he has supposed.

Ginter's prodigious research effort, wide reading in the secondary literature, and valuable clarifications regarding the pitfalls of the land tax duplicates should make this book an important reference. But the Olympian style with which he renders judgments about the work of others strikes me as needlessly tendentious; the author communicates a sense of engagement in a relentless polemic, however nominally grounded in a formal analysis. The vindication of G. E. Mingay's thesis regarding the impact of enclosure on the fate of small farmers, and more generally the upholding of his authority, appear ubiquitously as central subtexts. Methodologically, Ginter's emphasis on the primacy of error may also cause some unease.

For all of its apparent technical sophistication, the study does not transcend the use of simple descriptive statistics, such as means, medians, or ratios. The author describes vividly the predominance of variation or error, but he offers no advanced statistical method for minimizing it, so that greater use than he has allowed might be made of the abundant land tax

data. Ultimately, this book is a study about limitations, about what historians cannot do. I would have preferred a bolder effort to discover how the limitations of the land tax duplicates might be overcome, and a more positive emphasis on how their use might be applied more widely.

ALBION M. URDANK
University of California,
Los Angeles

DAVID HEMPTON and MYRTLE HILL. *Evangelical Protestantism in Ulster Society, 1740–1890*. New York: Routledge. 1992. Pp. xiv, 272. \$67.50.

The writing of evangelical history has become a growth industry of late as historians have come to appreciate the movement's ongoing significance. This splendid regional study by two Irish scholars sets a new industry standard. David Hempton and Myrtle Hill highlight the distinctiveness and the universality of Ulster Protestantism in a period of rapid social and political change, and they succeed in making comprehensible evangelicalism's peculiar and enduring impact on Ulster.

Part 1 explores evangelicalism's continental origins, its penetration of Ireland's Anglican and Presbyterian communities, and the emergence of Irish Methodism. The authors demonstrate that by 1800 Irish evangelicalism had succeeded in establishing a "new kind of associational, voluntaristic and non-creedal religion, serviced by itinerant preachers and committed to evangelism" (p. 19). Evangelicalism's strident anti-Catholicism, however, was more effective in shaping Ulster's Protestant identity than in converting Catholics. By 1850 both the Presbyterians and the Anglicans had adapted the voluntaristic evangelical forms to their pastoral and missionary work, but they had also increased their anti-Catholic zeal in order to reclaim their leadership role in the Protestant community. Evangelical backing for political and economic conservatism undermined Protestant class differences by stressing the interdependence of classes in what was understood to be a divinely ordained struggle against immorality and ignorance, the twin offspring of Catholicism.

Part 2 examines the contributions of evangelical voluntary agencies both to cooperation and schism as well as their wider effect on Irish society. Special attention is given to the "Second Reformation" of the 1820s, an unprecedented large-scale attempt to convert Roman Catholics, and the mixture of religious proselytism and cultural imperialism that this involved. The crusade led to a stronger, more vigilant (and thus apparently more threatening) Catholicism and to an increased concentration of Protestants in Ulster. It also destabilized conventional Catholic-Protestant boundaries while promoting both class harmony and denominational competition within Ulster Protestantism.

Part 3, "Culture and Society in Evangelical Ulster," concentrates on evangelicalism's urban mission and its remarkable appeal to women, who were always a majority within its ranks. Unfortunately, these themes, while inherently interesting and well developed, do not easily fit in with the flow of the rest of the work.

Part 4 explores the significance of the revival of 1859 in Ulster. Although the revival's religious impact was largely transient and its effects limited to the churches' existing constituencies, it did consolidate earlier gains and bolstered Protestant confidence. In so doing it helped to mold a provincial Protestant identity at the very time that Protestant hegemony was being dismantled. The continuation of Protestant solidarity required the opposing force of a strident Catholicism that Protestant aggressiveness had itself helped create, and it was nurtured by a historical tradition that found strength in adversity. Such a conflict helped Ulster Protestants overcome the denominational, class, and urban-rural tensions that sapped the strength of Christian communities in other parts of Europe and allowed them to maintain a distinct religious identity even as Irish Protestantism's political future became increasingly problematic.

This volume is well researched, carefully argued, and finely written. It will be invaluable to social, political, cultural, and religious historians.

DONALD M. LEWIS
Regent College

G. J. BARKER-BENFIELD. *The Culture of Sensibility: Sex and Society in Eighteenth-Century Britain*. Chicago: University of Chicago Press. 1992. Pp. xxxiv, 520. \$49.95.

The chronology of the development of separate spheres has become a vexed issue in the historiography of the eighteenth century. Why and when did eighteenth-century thinkers begin to regard women as utterly different from and opposite to men? When did the idea that women should remain in the private domestic sphere arise? Did the new stress on romance bring more equality to marriage or imprison women in sentimentality?

In this volume, G. J. Barker-Benfield at first seems to demonstrate that the eighteenth century opened up possibilities for middle and upper-class women's social and intellectual lives. He points out that new psychological ideas of the late seventeenth century, such as John Locke's philosophy of sensations, potentially enabled women to be seen as equal in intelligence to men, given the proper education. Increasingly, literate women could read and even write novels about feminine subjectivity, while a new genteel consumer society created heterosocial spaces where respectable men and women could mix. Barker-Benfield argues that the "dynamics of consumer capitalism" and the cult of "sensibility" "were perceived to make women and men characterologically

more like each other" (p. 278). As consumer culture became more feminized, middle-class masculinity began to incorporate the virtues of sensitivity as well as stoicism.

Barker-Benfield also demonstrates, however, how women and men manipulated, stretched, and reacted against notions of sensibility. Many commentators feared the feminization of culture, believing the luxuries of consumer capitalism would lead men to become soft and effeminate. Although effeminacy throughout the century connoted excessive heterosexual indulgence, Barker-Benfield might have noted more explicitly that by the late eighteenth century middle-class men often denigrated homosexuality and substituted the heterosexual role of husband and father for the bachelor rake in order to counter accusations that consumerism and sensibility would lead to male effeminacy.

The impact of sensibility on women's "self-fashioning" (pp. 82–83), as Barker-Benfield uses the term, was also ambiguous. Sensationalist psychology opened up women's potential, but also closed it down by describing women's nerves as fragile and more delicate than men's, different in every way. The cult of sensibility in novels enabled women to depict how men victimized them, for, as Barker-Benfield notes, eighteenth-century writers were "acutely aware" of gender conflict (p. xxviii). Yet, drawing on recent feminist criticism, he also shows that the requirements of chastity and propriety limited women's explorations of female sexuality and sexual conflict. And as Mary Wollstonecraft wrote, sensibility crippled women by forcing them to rely on emotion rather than reason, leaving them helpless and dependent on men. Indeed, Mary Wollstonecraft's simultaneous embrace and repudiation of sensibility runs as a leitmotif throughout the book, providing some of Barker-Benfield's most intriguing insights.

Barker-Benfield rejects a Foucauldian discourse-analysis approach in favor of a more old-fashioned intellectual history, allowing him to delineate the subtle ways in which individual male and female thinkers shaped, twisted, undermined, and reshaped eighteenth-century notions of gender and subjectivity. While he relies heavily on literary criticism and the works of other historians, he offers some original re-readings of texts by Bernard Mandeville, Adam Smith, Elizabeth Inchbald, and, of course, Mary Wollstonecraft. His arguments about sensibility and women's consciousness might have been more interesting, however, if he had tested the literary evidence against women's diaries and letters. But that would have been a different book.

Barker-Benfield's book would also have benefited from a stronger chronological sense and a more explicit discussion of how his ideas affect the historiography of separate spheres. Although he does not make this clear, his work could be used to demonstrate that throughout the eighteenth century, domesticity competed with more fluid and confused

notions of gender; only after the French Revolution did separate spheres decisively triumph as a middle-class ideology.

ANNA CLARK
University of North Carolina,
Charlotte

JAN GOLINSKI. *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760–1820*. New York: Cambridge University Press. 1992. Pp. x, 342.

This book traces the changing public character of chemistry in Britain from a distinctive Enlightenment style emerging in the 1760s to another, more typically nineteenth-century form of public engagement. According to Jan Golinski, the former, best exemplified by Joseph Priestley, stressed an active civic role for chemistry, a moral dimension to the science, and the democratic diffusion of socially useful research by egalitarian cadres of practitioners. The later type of public chemistry, epitomized by Humphry Davy at the Royal Institution, displayed the discoveries of great men—often using esoteric and expensive equipment—to more passive audiences in ways that underscored social, political, and theological conservatism.

Golinski presents a scholarly and fine-grained survey of the historical development and transformation of these different “public cultures” of chemistry. That survey ranges across diverse cultural contexts, from the Scottish university scene to dissenting English provinces to the metropolitan establishment in London. Rooted in the secondary literature, this account encompasses a wonderful variety of material, including obscure chemical and medical lectures and a fascinating story concerning Thomas Beddoes, the intoxicating effects of nitrous oxide (laughing gas), and the political and social controversies that swirled around Beddoes’s Pneumatic Institution at Bristol.

While not omitting relevant scientific and technical details, the result is an informed and informative social history of chemistry in Britain from 1760 to 1820. Golinski particularly succeeds in his goal of tracing the disciplinary development of chemistry as a profession in Britain in multifaceted and historically sensitive ways that avoid Whiggish retrospection. Fourteen superbly apt illustrations (portraits and contemporary engravings) enhance the presentation.

Golinski’s analysis is self-consciously and explicitly grounded in recent literature of the sociology of knowledge. To a degree, his commitments in this regard pay off in an enriched conception of his subject and his treatment of it. For example, his analyses of the technical and social success of efforts to impregnate water with fixed air (CO₂) or the failed program of eudiometry (testing the “goodness” of air with various instruments) are especially rewarding. His review of the “rhetorical” dimension of the conflict between Priestley and Antoine Lavoisier is also enlightening, if somewhat less original.

By the same token, Golinski’s identification with the sociology of knowledge leads to a fundamental confusion that undermines the coherence of his presentation. In particular he fails to distinguish clearly or consistently between two different senses of science as public culture. The phrase self-evidently concerns popular science and the consumption of science by the lay public and nonspecialist bodies. But the sociology of knowledge has concentrated on another sense, that of the more restricted but still public realm of the community of scientists who collectively validate claims of individual practitioners. Golinski’s is decidedly a study—and a good one—of science in public culture, but his own rhetorical bias is toward the arena of science itself as public culture. We are promised a great deal about the latter, but get more of the former. The result is confusing, as the focus vacillates uncritically and unsystematically between nascent communities of specialist chemists and the story of truly public audiences for chemistry.

Another problem concerns the relation between “science” and “experimental science.” In his opening pages Golinski makes the distinction, but soon drops it, rhetorically equating the two. Laboratory, experimental, and instrument-based chemistry may ultimately entail different kinds of historical relations with patrons and publics than other sciences, including not only abstruse and highly mathematical fields such as rational mechanics but even more accessible ones such as botany as well. One should not criticize authors for books they did not write, but Golinski’s work would have been strengthened had he restricted his language and the import of his claims to experimental science.

JAMES E. MCCLELLAN III
Stevens Institute of Technology

A. D. HARVEY. *Collision of Empires: Britain in Three World Wars, 1793–1945*. Rio Grande, Ohio: Hambleton. 1992. Pp. xvi, 784. \$85.00.

This intriguing and somewhat eccentric book provides stimulating but, in the last analysis, not entirely satisfactory reading. The idea of comparing Britain’s performance in the three great wars (1793–1815, 1914–18, and 1939–45) that marked its rise and decline as a global power is a very interesting one, but it is difficult to carry off not only because of the vast differences between the eras involved but also because the material to be mastered is so voluminous. (Paul Kennedy covered much of the same ground in *The Rise and Decline of The Great Powers* [1987] in a less entertaining but much more focused way.)

A. D. Harvey, whose previous work has largely been on the era of Britain’s wars against revolutionary and Napoleonic France, is in many ways at his strongest in treating this conflict. He has some interesting things to say about the long war’s effects on British attitudes, although that subject is dealt with

more fully and trenchantly in Linda Colley's *Britons* (1992). Harvey's treatment of Wellington's Peninsular campaign, particularly its logistics, is strong, and his suggestion that Wellington's views on rank-and-file soldiers shaped British army attitudes for the next century is almost certainly correct.

When, however, Harvey moves on to deal with the world wars of this century, the book's weaknesses begin to appear. He introduces an extensive comparative dimension to his narrative at this point, discussing at length, for example, the way other powers entered the war in an attempt to illuminate the British decision of August 1914. It is a bit difficult to see what light Portugal's entry into the war in 1917 can throw on Britain's. The result of these excursions, however, is that Britain itself disappears for pages at a time while Harvey discusses (usually quite interestingly) the politics of belligerency, civilian-military relations, and economic organization for war in a variety of other states. At times it seems as if a very different book is taking shape. When Harvey returns to Britain, his discussion of strategy and generalship, although often witty and certainly well-informed, does not really break any fresh interpretive ground. Douglas Haig is predictably (and correctly) hammered and the difficulty of integrating the new technology of destruction with the British army's tactical and operational thinking (not to mention its leadership style) is highlighted. None of this, however, is new. The section on the birth of military airpower is perhaps the best part of the treatment of 1914–18.

The flaws in Harvey's treatment of World War I are repeated in his treatment of Britain's 1939–45 swan song as a world power. He has some shrewd things to say about the strategic bombing of Germany, reckoning it a great waste of Britain's resources, a product of both the air staff's intellectual weaknesses and Arthur Harris's monomania—again, not new points, but very well put. Harvey's suggestion that one of Britain's major problems in waging World War II was the massive, inert, and unimaginative bureaucracy it built to manage total war, drawing on 1914–18 precedents, seems close to the mark. Harvey at times appears to feel sorry for Winston Churchill, an undeniably vigorous and imaginative leader hampered by the sputtering British war machine.

One issue connected to the operation of that machine that might have been fruitfully explored in connection with the strategic air offensive is British manpower shortages in the second half of the war. A month before D-Day the War Office told General Bernard Montgomery it could only cover British casualties for the first month of fighting, after which the British army would inevitably shrink. This brute fact was the backdrop alike to Montgomery's approach to battle and the dwindling influence of the British in the final months of the European war. Could the British have put larger armies in the field if they were not supporting the huge bomber offensive? Did Britain's total war bureaucracy mobilize people

efficiently only to use them ineffectively? This sort of question might with profit have been explored further within the framework of the synthesis Harvey is attempting. As it is, this facet of the British war effort needs more attention than it has hitherto gotten.

It is curious that in a book devoted to Britain's great wars, Harvey virtually ignores the Royal Navy that was surely crucial to ultimate victory in all three, even if in 1917 it first came close, through the sort of bureaucratic obtuseness that Harvey deplures, to losing World War I for Britain. Yet despite the impressively wide reading in secondary works on display in the footnotes and a not inconsiderable amount of archival work as well, this discursive, sometimes quirky, but always readable book is hard to recommend except as a highly individual commentary on Britain's experience of large-scale war over its 150 years as a great power.

RAYMOND CALLAHAN
University of Delaware

IAN DYCK. *William Cobbett and Rural Popular Culture*. New York: Cambridge University Press. 1992. Pp. xvi, 312. \$59.95.

In 1963, E. P. Thompson rescued the working people of Regency England from, in his now-famous words, "the enormous condescension of posterity." With this book, Ian Dyck substantially succeeds in doing the same for William Cobbett. Often vilified in his own lifetime and routinely misunderstood by later generations, Cobbett's proper place in the history of his era has never been firmly established—that is, until now. The man who emerges from Dyck's treatment is not entirely unrecognizable. Yet more than has been the case in the past, we are now able to approach a full appreciation of Cobbett's great humanity, devotion to principle, inexhaustible energy, and above all his true stature as an important nineteenth-century radical.

What for many has stood in the way of such exalted recognition are the contradictions and ambiguities in his character and public career. He has seemed at once heroic and prosaic, reactionary and radical, self-righteous and empathetic, bigoted and enlightened, egotistical and caring, pragmatic and deluded; the list could easily be extended. He was all of those things, to be sure, but Dyck shows with care and insight that the contradictions and ambiguities were more apparent than decisive. Despite the occasional suspicion that Dyck is a bit too ready to resort to special pleading on his subject's behalf, the reader comes away convinced that Cobbett's personal and political inconsistencies are a good deal less problematic when viewed from the perspective of the changing fortunes and conditions of his one abiding and beloved constituency, the agricultural laboring population of the southern counties.

In the course of what is best described as an intellectual and cultural history rather than a conven-

tional biography, Cobbett is presented as the embodiment of and spokesman for an agrarian underclass driven to despair and political consciousness by the wholesale expropriation of its labor, customary rights, and social values. His personal political odyssey may be seen, then, as one concrete manifestation of a class fault line that within two generations, from 1780 to 1830, split agricultural society into two distinctive and mutually antagonistic camps. In Dyck's reconstruction of events, all this constituted the rural version of a conflicted transition from vertical to horizontal social solidarities. This was a milieu that left precious little room for social harmony and deference, and in recognizing this with unmatched clarity, Cobbett became the only major radical figure to cast his lot with a segment of the working class that is too often ignored or, worse yet, denigrated.

Posterity's condescension weighs particularly heavy on the shoulders of the traditional Hodge stereotype, the regular depiction of the agricultural laborer as illiterate, apolitical, and animal-dumb. Like Cobbett, Dyck will have none of it. Listening with close attention and sympathy to the rural poor as they speak to us in the idiom of their songs, poetry, and broadsides, he uncovers the wellsprings of their deep disillusionment with those claiming social superiority over them, their dignity in the face of heartbreaking deprivation, and their determination to resist what appeared to them, as indeed it does to us, to have been rank injustice. In discussing the world of social and economic relations that gave rise to this legacy of cultural expression, Dyck is no doubt beholden to others whose work in the last twenty-five years has greatly enhanced our understanding of country life in early nineteenth-century England. He owes far fewer debts, however, in offering this world as the only legitimate context in which to assess William Cobbett. It is we who are indebted to Dyck for having written this humane and perceptive book about a fascinating man who is no longer quite the enigma he once seemed.

PETER DUNKLEY
Georgetown University

FRANÇOISE BARRET-DUCROCQ. *Love in the Time of Victoria: Sexuality, Class and Gender in Nineteenth-Century London*. Translated by JOHN HOWE. London: Verso. 1991. Pp. 225. \$29.95.

FRANÇOISE BARRET-DUCROCQ. *Pauvreté, charité et morale à Londres au XIX^e siècle: Une Sainte violence*. (Recherches politiques.) Paris: Presses Universitaires de France. 1991. Pp. vii, 245. 168 fr.

These two complementary studies in mid-Victorian British social and cultural history by a French scholar approach their subject from outside the conventions of Anglo-American social history. The results (as seen by someone from within these conventions) are mixed: on the one hand, a very casual use of refer-

ences and statistics, and a failure to acknowledge recent scholarly debates on many issues covered by the books; on the other hand, imaginativeness, interpretive freedom, and some wonderful storytelling, the latter a woefully underdeveloped art among social historians.

Pauvreté, charité et morale is a wide-ranging study of the Victorian literature on "the social question" at midcentury, centering on conceptions of the sexual morality of the poor. Françoise Barret-Ducrocq argues here that the crisis of conscience among the well-to-do which Gareth Stedman Jones (*Outcast London* [1984]) and many other historians see as a product of the 1880s was elaborated earlier and more gradually in the course of the century (p. 16), and that the systematic and "scientific" kind of charity offered by the Charity Organisation Society (COS) had predecessors in earlier generations. Whereas Stedman Jones focused his study of middle-class philanthropy on the COS as representing one strand of Victorian opinion, Barret-Ducrocq considers all kinds of writers and movements simultaneously, rather recklessly arguing that there is no great difference in approach between representatives of charitable organizations, state officials, and other social observers such as journalists and novelists. The author discusses all three groups of commentators and generally covers fairly familiar ground in doing so. I very much enjoyed the more archivally based and less familiar section on the actual charity givers, structured around case studies of organizations that include the London City Mission, the London Bible Women and Nurses Mission, the Salvation Army, and the Thomas Coram Foundling Hospital (among others). Rather than a conventional scholarly monograph, *Pauvreté, charité et morale* is best viewed as a thoughtful if opinionated essay on nineteenth-century charity and social thought.

Love in the Time of Victoria, first published in French in 1989, is intended as a more popular book, and indeed there is a new Penguin edition of the English translation. Barret-Ducrocq was one of the lucky few researchers who explored the archives of the Thomas Coram Foundling Hospital, with its fat case files concerning women who wanted to relinquish their illegitimate babies to the care of the hospital. Because the archive has been closed to researchers since 1980, *Love in the Time of Victoria*, a lively survey of its records, is extremely welcome. Because the female applicants were anxious to establish their relative innocence despite the extramarital pregnancy, the files are extraordinarily detailed on the circumstances under which they became pregnant and provide probably the closest look now available at the courtship and sexual lives of the Victorian poor.

Following a rather pat introduction claiming that their well-off peers viewed the sexuality of working people as "brutal animality" (p. 4), Barret-Ducrocq's approach is to classify and describe the foundling hospital cases (her sample is concentrated in the

1860s): servants seduced and tempted by gentlemen; rape or near-rape of servants within the confines of their mistresses' homes; working-class courtship and sexuality; abandonment; and so on. The author characterizes each kind of liaison and is generous and expressive in her retelling of these extraordinary stories. (A cable-TV miniseries seems all but inevitable.)

The social worlds of young working-class women, especially servants, come to life in these sketches. The cases demonstrate the incredible vulnerability of servants who, often utterly sexually ignorant, were prey to the men and boys of the house and could usually count on little support from their employers; or who, in relationships with men of their own class, could be suddenly dropped as the men panicked at the prospect of fatherhood. But the pregnancies are by no means all studies in victimization and passivity. Many liaisons reflect a servant's desperation for fun and diversion, or even her fantasies of being made a lady through contact with a roving gentleman. Many women also, in retaliation against men who had wronged them, initiated paternity suits or letter-writing campaigns exposing the men. Once pregnant, the women worked hard to get support for their child or to find a home for it; many eventually recruited the biological father's parents, or their own mothers, aunts, or sisters to raise the child while they resumed their work. The cases detail the networks through which the young women met their babies' fathers, the courtship patterns that structured the majority of these liaisons, and the long hours of work that kept young lovers apart.

In interpreting these rich documents for her readers, Barret-Ducrocq is a little disappointing. Rather than connecting with new work on sexual bargaining among poor women in the past, on the shaping of heterosexual desire, or on the meaning of fatherhood for working-class men, the author seeks mainly to demonstrate the contrast between the relatively patterned sexual lives of the poor and their contemporary reputation among policy makers for unmodulated vice, a valid line of inquiry but a limited one.

For an author desiring to "let the archives speak" (p. 181), Barret-Ducrocq's failure to give an identifying number or other archival reference (all the cases are cited simply as FH in the notes) is puzzling and unscholarly. And the statistics Barret-Ducrocq cites are also less than revealing. The total number of cases sampled is never revealed, so when we learn, for instance, that 29.5 percent of the women had relationships with their lovers lasting two years or longer we have trouble digesting the information, as we do with the other percentages offered in the book (pp. 87, 98, 112, 135). Among the most tantalizing of these missing statistics is the one noting, after many pages of riveting discussion on seductions or rapes of servants by middle-class males, that these are "only a minority" of the total (p. 74). *Love in the Time of Victoria* disappointed me as a scholar in British his-

tory, but kept me, as a lover of stories of the past, glued to its pages.

ELLEN ROSS
Ramapo College

EUGENIO F. BIAGINI. *Liberty, Retrenchment and Reform: Popular Liberalism in the Age of Gladstone*. New York: Cambridge University Press. 1992. Pp. xii, 476. \$74.95.

Between 1860 and 1880, a working-class movement of popular liberalism developed, eventually headed by Gladstone as charismatic leader. Eugenio F. Biagini makes major contributions to our knowledge of working-class culture, radical politics, and the nature of Gladstone's Liberal Party and demonstrates an innovative methodology using newspaper accounts and comparisons with Continental movements and models.

In an especially fine first section, Biagini describes the language of working-class politics: historical legitimacy conferred by proudly paraded roots in Cromwellian republicanism and Calvinist Christianity, with Chartism close by; kinship with Continental reform, especially the Risorgimento; conceptualization of the workers not as another class but as the nation (following the lead of Emmanuel Joseph Sieyès); and admiration for Switzerland and the United States. This litany, familiar in piecemeal form to anyone who has read working-class polemics, is beautifully presented here.

Biagini next discusses the views of popular Liberals on major reform questions, including fiscal policy, education, and church disestablishment. On fiscal policy he summarizes and adds to much recent scholarship, showing convincingly that Gladstone's budgets were paralleled by popular enthusiasm for retrenchment. On education and disestablishment he is less persuasive. Ratepayer support for Anglican schools was still a hot issue in the mid-1870s, but though Biagini is right to emphasize that nonconformist disestablishmentarians targeted the political and economic power of the Anglican church, it does not follow that doctrinal differences were unimportant. One of the many ways in which Gladstone resembled his popular supporters was his anxious and prayerful wrestling with fine points of theology. Indeed the rational quality of popular Liberalism derived in significant measure from the profoundly Calvinist struggle to understand the intellectual underpinnings of belief (or disbelief). Sliding by this point allows Biagini to suggest that the working class was more united than it was. Deep fissures existed when even the dates of meetings (the Sabbath versus a working day) automatically included or excluded whole groups of potential demonstrators.

Finally Biagini analyzes the role of popular Liberalism in the extension of the franchise and the election of new men to Parliament, concluding with a splendid section on Gladstone as a Weberian charis-

matic leader. He demolishes the idea that electoral reform sprang from a kind of insider trading. He clarifies the developing problems within the Liberal Party that were compounded by Gladstone's political style: as T. A. Jenkins in *Gladstone, Whiggery, and the Liberal Party 1874–1886* (1988) has suggested, rhetorical appeal could not substitute for grass-roots reorganization, the weakness of which Biagini brilliantly exposes. Most arresting is his comparison of British Liberalism with the Continental Left. He argues compellingly that Nonconformist religion moderated British radicalism, preempting strong Marxist or anarchist movements. With less justification, he likens Gladstone to European leaders such as Ferdinand Lassalle or V. I. Lenin. Although his evidence, new and old, for Gladstone's charisma is not in doubt, and his use of Weber is exemplary, the comparison is suggestive rather than convincing.

The problem ultimately is methodology. No one who has pored over decaying nineteenth-century newspapers can fail to be impressed by the depth and breadth of Biagini's reading, or by his mastery of the well-nigh overwhelming corpus on social and political history, not to mention the wide comparative perspective he brings. Newspaper accounts have been seriously underutilized until recently, although they give indispensable detail on what was said and done at meetings. They should be supplemented by petitions to Parliament, which are susceptible to quantitative analysis and permit more confident statements about exactly who was protesting. Finally, although he rightly avoids overuse of elite private papers, he could exploit them more. He reveals the almost physical empowerment Gladstone received from a good public meeting but does not show how Gladstone, a Christian with an acute sense of sin, agonized over this almost unbearable pleasure. Indeed, little attention is given to middle and upper-class reactions to popular liberalism. In some twentieth-century parallels, traditional elites had been discredited by military defeat. In Britain, they were alive and well, making the Liberal Party also a party of consensus that brokered different interests.

ANN POTTINGER SAAB
University of North Carolina,
Greensboro

RICHARD SHANNON. *The Age of Disraeli, 1868–1881: The Rise of Tory Democracy*. (A History of the Conservative Party.) New York: Longman. 1992. Pp. vii, 445.

Richard Shannon's long-awaited contribution to the Longman History of the Conservative Party covers a shorter timespan at greater length than its two distinguished predecessors in the series, Robert Stewart's *The Foundation of the Conservative Party, 1832–1867* and John Ramsden's *The Age of Balfour and Baldwin, 1902–1940* (both published in 1978). Perhaps as a consequence, Shannon's luxuriant narrative affords

many insights into the perceptions and actions of the Conservative leadership, but his analytic framework is too often submerged in the detail; depth of coverage has militated against breadth of perspective.

His point of departure is the Conservative Party's effort to come to terms with the major extension of the franchise Benjamin Disraeli had engineered in 1867, and he seeks to explain how, in the era of "Tory democracy," the party "became and remained . . . great, national, powerful and popular" (p. 4). The recovery in Conservative electoral fortunes, however, had little to do with any Disraelian commitment to or espousal of Tory democracy. The National Union, advertised by its proponents as a means of consolidating popular sentiment in the boroughs, was generally ineffectual, frequently ignored, and of marginal assistance to the Conservative electoral triumph in 1874. Formal party organization, while reformed during Disraeli's tenure, bore only a fitful resemblance to the superior machine touted by organizer John Gorst. Moreover, as Shannon persuasively demonstrates, Disraeli's own erratic leadership was undistinguished on such crucial issues as Ireland or the Anglican church. Disraeli's platform manner was also no match for his rival, William Gladstone; even in his oft-cited speeches at Manchester and the Crystal Palace in 1872, Disraeli was content with evoking a mood rather than prescribing a policy.

As an organizing principle, then, Tory democracy is of little use, and Shannon himself rightly stresses Disraeli's consistent and deeply held attachment to aristocratic government. Seeking to afford the country a respite from what he perceived as the unsettling impact of Liberal legislation, Disraeli and the party, fresh from an impressive victory, faced an unfamiliar dilemma: "they wanted Palmerstonian ends but they held in their hands Gladstonian means" (p. 196). If substance was thus subservient to gesture, it would be foreign affairs that offered the greatest scope for Disraelian flamboyance, and the party's more assertive posture in that sphere underwrote its claim to be great and national.

But to explain the party's growing domestic power and popularity is another matter, one complicated by the narrow chronological focus of this volume. The book's lack of a conclusion or a bibliography may be remedied in the companion volume Shannon has promised on the Salisbury era (1881–1902), but without any consideration here of the party's internal divisions in the early 1880s or Lord Salisbury's consolidation of power thereafter, it is difficult to set the party's growing popularity in perspective. Moreover, given the absence of any serious discussion of the press and the sometimes cursory treatment of the subterranean shift in voter preferences in many urban or suburban constituencies, one must presume that Shannon has deferred sustained consideration of some of the more intriguing issues in the politics of the period for his second volume. Indeed, some of the most incisive interpretive passages appear toward

the end of the volume, in a discussion of the legacy of the Disraelian era, an indication that the next volume will be worth waiting for.

In the meantime, Shannon's extensive use of relevant correspondence, his perceptive comments on elections, and his shrewd characterizations ensure that this book will prove rewarding to the specialist reader. For a general introduction to the topic, however, one might wish to supplement Shannon's book with Bruce Coleman's *Conservatism and the Conservative Party in Nineteenth-Century Britain* (1988).

FRANS COETZEE
Denison University

C. R. PERRY. *The Victorian Post Office: The Growth of a Bureaucracy*. (Royal Historical Society Studies in History, number 64.) Rochester, N.Y.: Boydell. 1992. Pp. viii, 308. \$79.00.

The story of the expansion of state intervention and the growth of government administration in nineteenth-century Britain has been at the center of an ever-growing body of historical literature in recent decades. Ideological and theoretical issues have enlivened the debate among historians who have sought to understand the dynamics of this growth and its relation to the prevailing climate of ideas, legislative action, economic and social developments, technological change, and the role of administrators and bureaucrats. In studies of particular areas of intervention (factory and mines regulation for example) and of individual government departments, historians have put forward a number of models or patterns to explain the growth of government in the Victorian period, and each of these models has engendered fresh debate and opened new avenues of research.

C. R. Perry's study of the Victorian and Edwardian Post Office is an important addition to this body of literature. His work is a thoughtful and well-documented history of a central-government department that experienced phenomenal growth and, perhaps more than any other branch of administration, came directly into almost daily contact with a large proportion of the population. At the same time, Perry's careful analysis of events, pressures, and personalities brings this book firmly into the context of the historiography of legislative and administrative growth.

In retrospect, the Post Office has been viewed as a success story. Its explosive growth following the adoption of the penny post and the postage stamp, its reputation as a money-making department of the government, its encouragement of thrift among the working class through the establishment of the Post Office Savings Bank, and its pioneering role in the development of nationalization through the acquisition of the telegraph and telephone systems, however, have masked the fact that its expansion was the result of the interaction of varied and complex circumstances and personalities. Indeed, as Perry points

out in his conclusion, the history of the Post Office is at once "convoluted" and "cloudy" (p. 272). There was no consistent linear development from one successful undertaking to another. The savings bank produced only modest revenues. Its foray, after 1864, into life insurance and annuities was a failure. Its telegraphic services, although popular, operated at an increasingly large financial loss. Parcel post, implemented as late as 1883, proved costly and placed the Post Office in an uneasy partnership with the railway companies. Its assumption of the telephone services was slow, hesitant, and reluctant.

The bureaucrats, G. Kitson Clark's "statesmen in disguise," played varying, sometimes inconsistent roles. Energetic administrative "zealots" (p. 272), such as Rowland Hill and Frank Ives Scudamore, were instrumental in moving the Post Office in new directions in the middle decades of the century. Their enthusiasm often outpaced the accuracy of their statistical and financial projections, but their success owed much to the coincidence of political and commercial circumstances that favored their proposals. By the end of the century the political climate, the Treasury's caution, and the absence of crusading bureaucrats led to a marked decline in the Post Office's activism.

Perry demonstrates effectively how conflicting ideologies could accommodate themselves to practical considerations, how theories of government and economics could be bent by pragmatic concerns, and how the competing interests of politicians, administrators, businessmen, and municipalities assumed varying configurations at different periods. In the end, Perry makes a compelling argument in this well-written study that no consistent single pattern of government growth over an extended period of time can account for the expansion of the Post Office. The Victorians, he writes, "to a degree sometimes overlooked approached the question of government growth on a case by case basis" (p. 120).

ROBERT M. GUTCHEN
University of Rhode Island

M. W. TAYLOR. *Men versus the State: Herbert Spencer and Late Victorian Individualism*. New York: Clarendon Press of Oxford University Press. 1992. Pp. x, 292. \$65.00.

M. W. Taylor's study of late-Victorian individualism, with special reference to the thought of Herbert Spencer, tellingly augments what has become a wealth of scholarship on the character of political argument in Britain during the last decades of the nineteenth century. Most of this scholarship has concentrated on those who sought an extension of state activity in the interest of greater social and economic justice. Those who from conviction opposed such extension have not gone unnoticed, but the remoteness of their position from the prevalent sensibilities

of the twentieth century, and the almost vestigial quality of their utterance even within the context of the late-Victorian period, have restricted the range and depth of scholarly consideration and sympathy they have evoked. For sympathy they will have to look to someone other than Taylor; from him they receive the measure of regard signified by a close and careful reading of their part in the debate.

His "central contention" is "that individualism represented a conservative adaptation of the liberal tradition" (p. 100). Although Taylor draws a distinction between Spencerian individualism and empirical individualism (Henry Sidgwick is taken as the chief exponent of the latter)—a distinction rooted in disagreement over epistemology and methodology—he observes that both strains issued in conservative political prescriptions. It has been generally recognized that in the political environment of the late nineteenth century an ideological commitment to individual liberty, private property, and free competition translated, albeit uneasily, into a practical endorsement of the existing distribution of political and economic power. This theme figures conspicuously in Edward Bristow's *Individualism versus Socialism, 1880–1914* (1987), a monograph Taylor does not cite. The signal contribution of Taylor's study lies in its cogent and persuasive rendering of Spencer's Synthetic Philosophy and of its ramifications in relation to his notion of society as an organism, and to his theories of progress, history, and the formation of character (Spencer, to be sure, saw these less as theories than as scientific laws). Furthermore, Taylor has superbly elucidated the difficulties the individualists (of the nonempirical sort) faced in their endeavor to extrapolate from the Spencerian conception of justice (embracing the "Law of Conduct and Consequence" and the "Law of Equal Freedom") an apology for the late-Victorian economic and social order.

Exception can be taken to certain lines of interpretation Taylor advances. A case in point is his virtually exclusive identification of the principles of mid-Victorian liberalism with the doctrines and legacy of the Philosophic Radicals (see his chapter 1, "Rival Claimants to the Benthamite Heritage"). This identification obscures the varieties of radicalism (and, indeed, nonradicalism) subsumed by mid-Victorian liberalism, and also gives to the "Benthamite heritage" an excessive prominence (A. V. Dicey's *Lectures on the Relation between Law and Public Opinion during the Nineteenth Century* [1905] notwithstanding).

If Taylor does not always convince, he seldom fails to do so. His book is a valuable addition to the literature on late-Victorian political thought.

BRUCE L. KINZER
University of North Carolina,
Wilmington

ANNE TAYLOR. *Annie Besant: A Biography*. New York: Oxford University Press. 1992. Pp. ix, 383. \$59.00.

The life of Annie Besant provides a compulsively good read. There is the sheer drama of her odyssey from High Anglican devotion to theosophical mysticism via materialism and free thought; the fascinating members of her supporting cast, including Charles Bradlaugh, George Bernard Shaw, Helena Petrovna Blavatsky, Mohandas Gandhi, and Jiddu Krishnamurti; the historical significance of the movements to which she contributed her zealous support, from neo-Malthusianism and socialism to Irish Home Rule and Indian nationalism. It would take a deplorably bad writer to make this material dull, and Anne Taylor certainly does not deserve that label.

Taylor offers the medium-length version of Besant's life, for those who want more than Rosemary Dinnage could encompass in her brief contribution to the Penguin "Lives of Modern Women" series (*Anne Besant* [1986]) and less than Arthur H. Nethercot furnished in his two-volume enterprise, *The First Five Lives of Annie Besant* (1961) and *The Last Four Lives of Annie Besant* (1963). Taylor's book is a fast-paced narrative, based on thorough archival research, which retraces familiar ground but nonetheless offers a persuasive interpretation of its subject. In Taylor's hands, Besant emerges as a power-hungry egotist, fundamentally proud, willful, and self-important, tormented by a compelling need for spiritual reassurance. The author does not deny that Besant felt passionate outrage over the suffering of the poor and disadvantaged wherever she encountered them, but Taylor sees her efforts on their behalf as part of Besant's ongoing quest for admiration, influence, and authority. As for Besant's affiliation with a long list of male mentors, Taylor crisply observes that she was "always susceptible to men who acknowledged her intellect" (p. 47).

The book is less convincing when the focus strays from Besant, for Taylor has merely sketched the background needed to frame her extraordinary life. Numerous men and women make cameo appearances, with only the briefest of introductions to explain their particular function on the scene. Some receive no introductions at all. Elizabeth Cracknell, for one, turns up halfway through the book as Besant's companion, whose desertion to marry an army officer in 1885 inconvenienced Besant, but where Cracknell came from and whether she was a salaried employee or personal friend is never explained. A similar mystery shrouds "Miss Willson," who arrived in Ceylon with Besant in 1893 and "shared her Indian adventures for many years" (p. 267), but who never figures again in the book. Occasionally Taylor seems not to realize that she is handling well-known themes in British social and political history. For example, Charles Bradlaugh's opposition to an eight-hour working day in 1888 on the "grounds that no limit ought to be put by government to an individual's right to manage his own affairs" was hardly "idiosyncratic" (p. 206) but rather squarely within the British liberal tradition, which he, in many respects, exem-

plified. It also comes as a surprise to find John Maynard Keynes signing a petition in 1883 (p. 346, n. 27)—the year of his birth.

The need to cram Besant's incessant and varied activity within the confines of a manageable single volume doubtless required Taylor to compress much of her story. As a result many of the transitions are bewilderingly abrupt. It seems, in fact, as though the book has been edited rather ruthlessly, for the end-note citations do not always fit the material to which they are attached in the text. Thus, while Taylor has offered a psychologically coherent portrait of Annie Besant, one still must turn to Nethercot for many of the details and explanations that fill out the canvas.

JANET OPPENHEIM
American University

JOHN D. FAIR. *Harold Temperley: A Scholar and Romantic in the Public Realm*. Newark: University of Delaware Press. 1992. Pp. 362. \$55.00.

Harold Temperley was a Cambridge man through and through: an undergraduate at King's College and a lifelong Fellow, and latterly Master, of Peterhouse. His early career reads rather like what contemporary professors might find an unpromising case for tenure; his first book, rushed to print, was a jejune disaster and he was a terrible lecturer. Yet Temperley overcame it all and went on to establish a brilliant reputation as a historian of nineteenth-century Britain and Europe. He was a monumental figure, the successor to Acton in the establishment of the still-flourishing Cambridge school of history.

What makes this biography particularly interesting are two aspects: first, John D. Fair develops in great detail the argument hinted at in the subtitle. Temperley was without doubt an unashamed romantic: he believed that spirit, the unseen, governed the world as much (or even more) than the mere physical or visible nature of things. Early on, he developed a taste for romantic literature, particularly that of Eastern Europe, which much influenced his view both of that troubled locale and of the proper way to do history. This idiosyncrasy colored his teaching, his massive published output, and even his taste for communicating to those close to him through particularly awful poetry.

Temperley's romanticism was demonstrable also in his cherished belief that intellectuals could and should take active parts in public life in order to set the world aright. Nowhere is this clearer than in the fact that he gloried in his time as an adviser to British government during the Paris Peace Conference following World War I.

A second characteristic emphasized by Fair is that Temperley never sacrificed a deeply cherished liberal belief that history should be written not as a study of the past for its own sake but as a "school for statesmanship." It is ironic that his most prized pupil was

Herbert Butterfield, who later became the most noteworthy critic of this "Whiggish" perspective.

This is an excellent study that will hereafter complement the biography of Temperley's co-editor of the pre-1914 British diplomatic documents, Frank Eyck's *G. P. Gooch: A Study in History and Politics* (1982). While it is well known to Fair that he and I are unlikely ever to agree on the degree of influence Temperley's works had on Neville Chamberlain during the darkest days of appeasement, this book should be read by students and teachers of history who seek insights into the fascinating life of one of those who materially shaped our craft and a shrewd analysis of the development of the history profession in a particularly definitive period. It is a good read: anecdotal without being gossipy, and respectful of the works of a great historian (and apparently an infuriatingly difficult man) without being hagiographical.

R. J. Q. ADAMS
Texas A&M University

JOHN KENDLE. *Walter Long, Ireland, and the Union, 1905–1920*. Buffalo, N.Y.: McGill-Queen's University Press. 1992. Pp. xi, 246. \$49.95.

Walter Hume Long was one of the most important English unionists in the early twentieth century, and the union was his primary concern in British politics between 1905 and 1920. John Kendle forcefully argues that throughout his political career, Long, as much as unionists Arthur Balfour, Sir Edward Carson, Andrew Bonar Law, Lord Selborne, and Austen Chamberlain, or for that matter liberal Prime Minister David Lloyd George, was a principal architect of the partition of Ireland and, therefore, of the constitutional remapping of the United Kingdom itself in the early 1920s.

The relation of Walter Long to Ireland has received little attention from historians. Long's own *Memories* (1923) and the biography by Sir Charles Petrie (1936) are largely unrevealing, and Richard Murphy's Ph.D. dissertation (1985) deals with Long's entire career. Kendle concentrates his analysis instead on Long and Ireland in these fifteen critical years just prior to partition in 1920–21. He begins by examining Long's upbringing and early career. Included here are details about his "country life" in Wiltshire, schooling at Harrow and Oxford, and his introduction to the House of Commons in 1880. This was the beginning of a unionist political career that lasted until 1921, sixteen years of which he served as a cabinet minister. Prior to 1904, Long was deeply involved with local government legislation. He also spent considerable time in Ireland. In 1889, he became president of the new board of agriculture, although his interest in local government and Ireland continued. In 1905, shortly before the fall of the unionist government, he became chief secretary for Ireland.

As chief secretary, he held firm to unionist principles and, when the union was in danger from Home Rule in 1912–14, he staunchly defended it. Countering Lloyd George over Home Rule in 1916, he firmly established himself during the next four years as a key figure in the British government's Irish policy. Convinced during this period that an alternative to Home Rule would have to be found, Long came to advocate a federal solution for the whole United Kingdom, not just Ireland. Unfortunately, the Anglo-Irish War from 1919 to 1921 made his federal solution as well as Home Rule impossible. Instead, in order to restore order in Ireland, the Government of Ireland Act was passed in late 1920. This act, "devolution in a federal form," offered Ireland two legislatures—one north and one south—and was Long's final contribution. Rejected in the south, this act along with the Anglo-Irish Treaty of 1921 partitioned Ireland and remapped the United Kingdom.

This is a well-researched book in which Kendle affords the reader an easy window to his sources. A brief bibliographic note and index are also included. Long's own papers are the single most important manuscripts used and largely involve the years 1905–20. The majority of them are housed in the Wiltshire County Library, but a small collection of letters are at the British Library.

Considering the importance of unionism and Ireland in British politics, it is surprising that so little has been written on this important topic. Kendle has made a significant contribution, but there still remains the need for a definitive synthesis of British unionism for these important years. When this is attempted, I hope the person who undertakes it will not go too far with the idea that the Irish settlement in 1920–21 was essentially a unionist gift. Such a notion diminishes the significance of the Irish part of the conflict between 1916 and 1921. For the present, any verdict on the web of significance that was woven in British-Irish relations in the early twentieth century remains tentative.

MARTIN F. SEEDORF
Eastern Washington University

STEPHEN JEFFERY-POULTER. *Peers, Queers, and Commons: The Struggle for Gay Law Reform from 1950 to the Present*. New York: Routledge. 1991. Pp. xiii, 296. \$14.95.

Historians have often ignored gender and sexuality in analyzing political phenomena. When researching a study of Oscar Wilde a few years ago, I was surprised to find that leading accounts of the Liberal Party in England in the 1890s included no mention of the Wilde trials. Since the politics of scapegoating are unlikely to cease, historians have a responsibility to fill the lacunae.

In studying efforts to decriminalize male-male sexual activity in Britain since 1950, Stephen Jeffery-

Poulter has drawn amply on news accounts, statements by politicians, and a variety of materials produced by lesbian and gay groups. The result is a useful, provocative, and often entertaining book. On the analytic side, he is less successful. Jeffery-Poulter demonstrates that during the 1950s some parliamentarians, churchmen, and leading journalists supported reform of the Criminal Law Amendment Act of 1885, the most regressive anti-homosexual legislation in existence in Western Europe (excluding the Republic of Ireland). Other members of the same groups stalled changes until 1967, a full ten years after the Wolfenden Committee had recommended partial decriminalization of male homosexual activities. Why was the establishment so divided on this issue? Jeffery-Poulter, immersed in detail, does not provide an answer. And he does not sufficiently take into account factors such as the pressure that the U.S. State Department exerted early in the 1950s to have homosexuals ousted from security-sensitive positions (p. 25).

In later chapters, a teleological narrative becomes evident. According to Jeffery-Poulter, recent setbacks, especially the passage of Section 28 of the Local Government Act of 1988, have resulted in the mobilization of British lesbians and gay men on a mass scale for the first time (p. 234). Out of adversity has come enhanced group consciousness and solidarity (p. 260). The ideological ground on which reform will be debated in the future likewise has shifted from the defense of privacy urged in the 1950s and 1960s to a defense of the civil rights of members of sexual minorities (p. 240).

Article 28 states that "A local authority shall not . . . promote the teaching in any maintained school of the acceptability of homosexuality as a pretended family relationship." When originally proposed by Lord Halsbury in 1986, this legislation was opposed by the government spokesman in the House of Lords (p. 211). But in the spring of 1987 Prime Minister Margaret Thatcher publicly endorsed it. It became law in March 1988. Jeffery-Poulter tracks the mix of double-think, partisan calculation, and ideological bias that contributed to this series of events.

The combination of appeals to "family values" with a targeted attack on a particular minority group will be familiar to analysts of American politics who are puzzling over the decision taken by the Bush campaign early in 1992 to target male homosexuals. Did Republicans think that what had served the Tories would also serve them? (See Paul Cellupica, "The Political Dawn Arrives for Gays," *New York Times*, November 7, 1992, L 21.) The tactic did not work. But the struggle between Joseph Fernandez, chancellor of the New York City public schools, and a local school board in Queens over the board's refusal to implement a "multicultural elementary-school curriculum that includes controversial passages fostering respect and appreciation for gay people" (*New York Times*, December 15, 1992, B 5), indicates the sensi-

tivity of the issue and its continuing susceptibility to exploitation.

Jeffery-Poulter's research was facilitated by the existence of the Hall Carpenter Archive at the London Lesbian and Gay Centre. Subsequent funding cuts resulting from the demise of the Greater London Council (GLC) have led to "splitting up" (p. xiii) the archive. This by-product of the government's abolition of the GLC and other metropolitan councils should give historians cause for concern, since the history of one group can be told only in relation to other groups. The loss of this resource, then, not only affects lesbians, gay men, and others interested in the history of sexuality and gender but it also prompts distortions and gaps in historical accounts.

Unfortunately, this problem also exists in Canada and the United States, where libraries have let slip opportunities to house local materials, often on the grounds that the library in question does not have a policy of collecting archival materials pertaining to sexual minorities. On the basis of this sort of logic, materials gathered by individuals and groups on an ad-hoc basis will continue to be permanently lost.

RICHARD DELLAMORA
Princeton University

DONNA BOHANAN. *Old and New Nobility in Aix-en-Provence, 1600–1695: Portrait of an Urban Elite*. Baton Rouge: Louisiana State University Press. 1992. Pp. xi, 168. \$27.50.

Donna Bohanan offers to solve, in 137 pages, a problem already definitively treated by Monique Cubells, first in a five-volume *thèse d'état* and then summarized in the splendid volume *La Provence des Lumières* (1984). The problem is the social position, among the elites of the *ancien régime*, of the *parlementaires* of Aix. Did they constitute a coherent and self-conscious group? Were they perceived as equals by the *noblesse* of Provence?

Barely taking in the evidence accumulated by Cubells or by Sharon Kettering in her fine monograph, *Judicial Politics and Urban Revolt* (1978), Bohanan blunders into every trap laid for the unwary neophyte. Her thesis appears to be that there was no *noblesse d'épée* in Aix, and therefore no *noblesse de robe* either (p. 22). Although she recognizes that among the *parlementaire* families "many could not claim chivalric origins" (p. 22), it would be more accurate to say that few—at most two out of 163 families by the end of the seventeenth century (Cubells, p. 39)—could legitimately make such a claim.

Since there is no way of opposing *robe* and *épée*, the author settles for an arbitrary distinction between what she calls "old" and "new" families: those ennobled (or having usurped titles) before 1500 and the rest. By comparing these groups she hopes to answer the question of whether there were conflicts based on the antiquity of a family's status within the *parlemen-*

taire elite. To answer this question, she selects five families, two meant to represent the older families and three the newer. As it happens, the families chosen as representatives of the "old nobility" are no more authentically noble than the others. The Coriolis family, for example, in spite of its "immemorial" claims to nobility "extending back to the Gallo-Roman aristocracy" (p. 22), was ennobled only in the sixteenth century (Kettering, p. 217). As for the families representing the newer nobility, at least one of them was condemned for usurpation in 1667 and cannot, therefore, be counted as nobility. In sum, the five families Bohanan selects were all pretenders in some sense.

Not surprisingly, she finds few real distinctions between the "old" and "new" families as she looks at their marriage and investment strategies. She would seem to confirm Cubells's conclusion: "the High Robe is revealed as being unified, autonomous, solidly constituted as a group." This conclusion, however, is meaningful only if one adds, as Cubells does: "vis à vis de l'Épée" (Cubells, p. 397). The distinction between *robe* and *épée* in seventeenth-century Aix was largely a cultural, not a legal, distinction. Bohanan seems to grasp this occasionally, but the few pages she devotes to culture and education as a kind of afterthought are not founded on even the most elementary research.

GEORGE HUPPERT
University of Illinois,
Chicago

JEAN-MARC MORICEAU and GILLES POSTEL-VINAY. *Ferme, entreprise, famille: Grande exploitation et changements agricoles; Les Chartier, XVII^e–XIX^e siècles*. (Les Hommes et la Terre, number 21.) Paris: L'Ecole des Hautes Etudes en Sciences Sociales. 1992. Pp. 397. 150 fr.

Now that French historians have given up trying to find an English-style agricultural revolution in eighteenth or nineteenth-century France, Jean-Marc Moriceau and Gilles Postel-Vinay in their superb monograph have chosen to get at the continuities and discontinuities of French farming by analyzing over a period of almost 300 years (ca. 1600–ca. 1870) the careers of a single (and apparently typical) dynasty of large-scale farmers, the Chartier family, established in the Pays de France, the region just north and northeast of the capital. The authors' choice fell on the Chartiers not merely because their career can be traced back to 1404 and fully documented from the late sixteenth century onward but also because the family was unusual in retaining enough account books (*livres de raison*) from the seventeenth through the nineteenth centuries to complement the more formal documentation in the ubiquitous notarial archives. The results are an impressive, multifaceted study presented with elegance and intelligence that is unobtrusively (although specialists would have wel-

comed a full listing of sources) based on exhaustive and exhausting archival research.

The authors begin by summarizing the careers of the successive Chartier dynasts, whose power and prosperity, until the French Revolution opened new vistas, rested on their role as major tenant farmers for powerful ecclesiastical landlords. They then turn to a demographic and sociological analysis of the family system that, in a region of equal inheritance among all children and in a formidably prolific family, proved extraordinarily successful in maintaining each wave of offspring in something like the station into which they were born. The authors analyze the stages of family enrichment and the acquisition of family property, and then examine the technical aspects of the Chartiers' farming over time, a complex evolutionary process in which progress does take place, yet not in terms of increasing wheat yields, the region's chief cash crop. The authors end by considering the changing role of the Chartier farms in the market economy and the family's evolving incomes, savings, and indebtedness, which, by the nineteenth century, carried the Chartiers from the precarious prosperity of the late seventeenth century to unabashed riches 200 years later.

How do Moriceau and Postel-Vinay account for this elite farming family's rise over time to great wealth, when during the same period the production of wheat, its main crop, remained virtually stagnant? Four crucial variables come into play: the rent the Chartiers had to pay, the cost of getting their many children properly established in independent households, the workaday costs of producing traditional crops, and, finally, the opportunities for marketing new farm products.

The rent the Chartiers paid to their church landlords was always heavy (and critically so during prolonged crisis such as the period 1690–1710, when, for example, a collateral branch of the family lost out and sank into obscurity), yet grain prices did outstrip rent increases in the second half of the eighteenth century, easing the burden. In the seventeenth and eighteenth centuries, with as many as twelve and even fifteen children, 50–70 percent of whom survived to maturity, the pressure to assure each child a fair stake in the family's worldly fortunes (including expensive educations, dowries, settlements, and advances) was tremendous, dictating the paterfamilias' strategy of land acquisition and "cash-flow." As to actual agricultural operating costs, the Chartiers seem to have achieved a steady decrease of labor costs per hectare from at least the beginning of the eighteenth century, largely the result of land consolidation. Finally, in terms of new markets, in the eighteenth century the Chartiers, by entering into direct agreements with great Parisian aristocratic households, were able to tap the lucrative urban market for litter straw for the growing horse population while also obtaining horse dung for manure at a minimal cost.

Although the French Revolution disrupted such

profitable arrangements, it created more spectacular opportunities for the Chartiers. In the 1790s, they were able to purchase the properties they had farmed for the church and, thanks to inflated paper money, to do so at a bargain price. This change from tenancy to proprietorship largely explains their leap from prosperity to wealth, yet the nineteenth-century Chartiers' conversion to family limitation surely also helped enhance incomes. Whereas in the seventeenth and eighteenth centuries between four and eight heirs had to be satisfied each generation, in the nineteenth century it was two or three, although this saving was partly offset by the older generation's earlier retirement. Finally, aided by investment in farm machinery, by new techniques, and by generally lower land taxes, agricultural production costs continued their decline. The growing gap between stable gross income and declining outlay added up to rapidly rising family wealth.

This brief review cannot do full justice to a demanding and complex yet most rewarding monograph with significant implications far beyond one family's fortunes. It makes indispensable reading for anyone interested in the evolution of French agriculture, of rural elites, and of family strategy among dominant groups.

PETER H. AMANN
University of Michigan,
Dearborn

ERICA HARTH. *Cartesian Women: Versions and Subversions of Rational Discourse in the Old Regime*. (Reading Women Writing.) Ithaca: Cornell University Press. 1992. Pp. xi, 267. Cloth \$38.95, paper \$13.95.

In this challenging and immensely valuable book, Erica Harth pushes the triad of gender, science, and intellectual milieus (salons, academies)—so prominent in recent revisionist studies of early modern intellectual life—onto new conceptual and interpretive ground. Harth shows a succession of women writers, from the *précieuses* to Olympe de Gouges, confronting and contesting what she identifies as the "Cartesian discursive complex": scientific objectivity, rationalism, universalism.

Harth's purpose is to historicize "the uneasiness of the relationship between educated women and rational discourse" (p. 14) and to establish a genealogy for twentieth-century visions of feminist science and current feminist epistemologies. She makes a generally persuasive case that the deficiencies of rationalism recognized now, at the waning edge of modernity—the divorce of science from values and the ill-conceived pursuit of objective observation—were discerned and countered by women writers at the outset of the paradigm shift before a hegemonic "Cartesian" discourse silenced their voices.

According to Harth, the "feminist alternative" put forward in the formative, seventeenth-century phase

of the new discourse tried to take the fundamentals of Cartesianism down a different pathway, to a form of modernism capable of recognizing that the particularities of the subject-observer are incorporated into knowledge, that value is embedded in the "facts" of nature, and that equality can be grounded in an appreciation of gender difference. Harth suggests that this is, for example, what the *précieuses* were advancing when they used a more traditional metaphorical language of beauty to express the clear truths of scientific realism.

Harth traces how the feminist alternative lost out, over the course of a century, to an idiom that, while professing universalism, was profoundly masculine and exclusionary. Eventually, after Olympe de Gouges's futile attempt to use the discourse of abstract universal human rights for pressing claims based on the physically embodied differences between the sexes, women abandoned the struggle with rationalist discourse in favor of a separate culture based on feminine sentiment.

The salons, so often hailed as feminocentric venues that empowered intellectual women, were, Harth argues, "a discursive dead end for women" that kept them in a subordinate role and apart from the academies where the new discourse was created and shaped. The quarrel over the salons was not (as I argued in *Le Paradis des femmes: Women, Salons, and Social Stratification in Seventeenth-Century France* [1976]) social politics but a gendered contest over control of discursive development and women's access to serious intellectual work.

Harth's interpretation is well grounded in recent literary scholarship, philosophically astute, and replete with independent judgments and original insights. It lends new depths of meaning to the women's works it engages and new precision to characterizing salons and academies. Her unifying concept of "Cartesian discourse" works tolerably well to stand for the set of rationalist presuppositions that structure modern thought.

But Harth's titular conception of "Cartesian women" is less successful. She defines the term carefully—not as disciples of Descartes but as the women who struggled, even as they more or less adopted rationalist discourse, to incorporate subjective experience and ethical concerns into its construction of knowledge. This special meaning of the term "Cartesian women" becomes, when applied to eighteenth-century writers who are Cartesian in no accepted sense, so arcane that it obscures the set of writers it is meant to identify.

Nor does Harth's limited historical contextualizing of the literary and philosophical texts allow her to raise issues of explanation. Why were the social sites of discourse formation so strongly gendered? What matrix of intellectual values or social claims motivated those for whom it was so important that the academies remain male? Why—for what philosophical or social or other reasons—did Cartesianism narrow and

close rather than move toward "the feminist alternative"? Words compose a discourse, but men and women step to nonverbal drummers that cannot be teased from the internal dynamics of writings alone.

CAROLYN CHAPPELL LOUGEE
Stanford University

MARTIN PAPENHEIM. *Erinnerung und Unsterblichkeit: Semantische Studien zum Totenkult in Frankreich (1715–1794)*. (Sprache und Geschichte, number 18.) Stuttgart: Klett-Cotta. 1992. Pp. xii, 353.

During the last thirty years scholars in several disciplines have explored and transformed the history of death in early modern France by using new sources and methods to investigate old questions more systematically and creatively than before. Pierre Goubert and François Lebrun used parish registers as well as records of grain prices and medical interventions to chart the incidence of death in space and time. Michel Vovelle and Pierre Chaunu used wills to document synchronic differences and diachronic changes in attitudes toward death. Against the background of the encyclopedic but impressionistic work of Philippe Ariès, meanwhile, Jacqueline Thibaut-Payen explicated the juridical and jurisdictional issues, and Robert Favre exhausted the philosophical and literary texts. Since John McManners synthesized research on mortality and mentalities, Richard Elin and George Armstrong Kelly have studied the evolution of cemeteries and the political meanings of death, respectively, before and after 1789.

Historians have repeatedly assessed the impact of the philosophes, as well as epidemics, on eighteenth-century feelings about the representations of death. Martin Papenheim, like many of his predecessors, locates his work within the conventional framework of the confrontation between the *ancien régime* and the Enlightenment, followed by the revolution. He cites some sociological and anthropological literature and describes his book as an analysis of discourses, but his "semantic studies" are not informed, or deformed, by theoretical agendas. Papenheim's bibliography, oddly enough, does not include Kelly's eccentric but suggestive *Mortal Politics* (1986), which, of all the books mentioned above, most resembles his own. A few subjects included by Papenheim have already been discussed more thoroughly elsewhere, but the three sections of this book provide an instructive, if somewhat schematic, account of changes and continuities in the rituals and rhetoric of death.

Traditional, philosophic, and revolutionary funerals and eulogies not only remembered the dead but also read their lives in distinctive ways for the edification of those who mourned them. Under the *ancien régime*, services and sermons for members of the royal family and court nobility acknowledged the accomplishments but emphasized the virtues and piety of the deceased in order to effect the regeneration of the

living. At the same time, they naturalized and sanctified the established order of things, based on social inequality and political subordination. Papenheim rightly discerns tensions within traditional discourse by the time of the death of Louis XV in 1774. Many eulogists criticized the king's profligacy, and some adopted unconventional language popularized by the religious and constitutional conflicts that spanned his reign. Several years later the republic of letters lamented the death of Voltaire in philosophic style, celebrating his genius and humanity. The philosophic discourse emphasized the talents and achievements of artists, scientists, and, especially, writers. Academic eulogies and orations focused on reform in this world rather than salvation in the next. They invoked the authority of public opinion and awarded immortality to Frenchmen, regardless of rank, who promoted the welfare and advanced the enlightenment of the kingdom.

The revolution, which claimed Voltaire for itself, reshaped the rituals and rhetoric of death in its own image for didactic purposes. Revolutionary discourse, premised on natural equality and national sovereignty, continued the related processes of democratization and secularization already underway, as Papenheim shows, before 1789. Funerals and eulogies honored famous Parisian politicians along with unknown provincial patriots who sacrificed themselves for the fatherland. They intended to dechristianize death but also appropriated the language of martyrdom. They helped to consolidate the new order of things but also reflected the changing course of the revolution, which admitted heroes to (and on occasion expelled them from) the Pantheon.

JEFFREY MERRICK
University of Wisconsin,
Milwaukee

LUIGI BLANCO. *Stato e funzionari nella Francia del Settecento: Gli "ingénieurs des ponts et chaussées."* (Annali dell'Istituto storico italo-germanico: Monografia, number 14.) Bologna: Mulino. 1991. Pp. 450. L. 40,000.

Luigi Blanco sticks closely to the theme announced by his book's title. In six chapters he moves from corporate society and administrative monarchy to the service of the "ponts et chaussées," then to their functional administration, the provincial administration, the communications routes, and finally to engineers as an elite group. The most original material is about the provincial administration and the engineers. Regarding the former, Blanco shows how the engineers had to contend with widely differing conditions in the *pays d'états* and in the *pays d'élections*. His chapter on the engineers as an elite is also well done, demonstrating the remarkable growth of the engineer corps from 1747 onward.

Blanco has not been well served by his publishers, who have allowed him little in the way of graphic

material. Surely for such a theme it would have been good to have shown by maps and drawings some of the vast public works projects undertaken in France at this time, thus giving the book the kind of visual appeal of Ann Blanchard's rich and beautiful book on *Les ingénieurs du "roy" de Louis XIV à Louis XVI* (1979). Still, within its limits, this is a useful contribution to an important and rather neglected area.

DAVID BUISSET
Newberry Library,
Chicago

STEVEN G. REINHARDT. *Justice in the Sarladais 1770–1790*. Baton Rouge: Louisiana State University Press. 1991. Pp. xxi, 301. Cloth \$39.95, paper \$16.95.

Steven G. Reinhardt has examined justice in the *Sénéchaussée* of Sarlat from 1770 to 1790. Sarlat, a market town of about 5,000, was the administrative center of a *sénéchaussée* whose population numbered approximately 116,000. Located in the Périgord, the Sarladais was rural, impoverished, and relatively isolated. The weather was unpredictable, the soil generally was poor, and commercial activity, although increasing, was negligible. Famine, epidemics, and a growing exodus of able-bodied inhabitants completed the bleak picture.

In this traditional corner of south-central France, the three systems of justice—royal, local or popular, and seigneurial—that had evolved in the *ancien régime* were both complementary and competitive. Reinhardt's book is particularly useful in explaining each of the three systems of justice and describing the attractions of each.

As the French monarchy matured and became increasingly centralized, its reach lengthened and grew stronger. Attempts to exert control from Versailles were aided by the army, by royal agents and the intendants, by improved roads and canals, and by an expanding system of royal justice that found its efforts to centralize authority thwarted to some extent by the existing system of popular justice and seigneurial courts.

Whether administered by the seigneur or by the less formal popular system, local justice was used to settle minor disputes and infractions of community standards and to restore "social equilibrium through arbitration and measured, often ritualized violence" (p. xviii). Local justice relied on traditional practices and buttressed the hierarchical social structure and the "moral economy" of the popular classes. The seigneurial courts also defended and enforced traditional prerogatives. The establishment of "foreign" royal courts was therefore seen as an intrusion and, in most instances, was resisted.

Royal justice challenged traditional methods of settling disputes by enforcing the king's law. The urban and rural bourgeoisie increasingly turned to royal justice, a development that signaled the "evolu-

tion of the rural community and its relations with the changing exterior world" (p. 270). Reinhardt has found that greater numbers of peasant proprietors and sharecroppers also began to use royal justice because it promised not only protection and recourse in disputes with their landlords but impartiality as well. "The general breakdown of traditional habits of deference and submission may have been affecting the Sarladais, as new ideas of equality and greater geographic and social mobility favored the rise of an egalitarian spirit" (p. 272).

Reinhardt notes that the desire of the community to protect its cohesion was reflected in the judicial system. Even royal courts were not immune from this influence, and they were forced to recognize it in their decisions. In the traditional informal system of dispute settlement, as well as in the royal courts, it was understood that there was a certain "threshold of toleration" beyond which the community demanded punishment. "Historians of Old Regime France who study penalties in relation to crimes should therefore proceed with caution: A penalty often reflected a desire to punish an entire history of delinquent behavior as well as the crime of the moment" (p. 131).

Reinhardt has demonstrated that the modern world was intruding even in a rural backwater such as the *Sénéchaussée* of Sarlat. After 1789, more private individuals were likely to use the royal courts. Modern ideas of personal property and individual rights led to the rejection of traditional communal rights such as grazing or gathering and of community practices such as the *charivari* or the exaction of vengeance. This well-written study is an important addition to the literature on the development of the judicial system in the *ancien régime* and contributes to our understanding of the complex interaction of forces on the eve of the revolutionary era in France.

THOMAS F. SHEPPARD

College of William and Mary

MICHAEL MEINZER. *Der französische Revolutionskalender (1792–1805): Planung, Durchführung und Scheitern einer politischen Zeitrechnung*. (Ancien Régime: Aufklärung und Revolution, number 20.) Munich: Oldenbourg. 1992. Pp. 307. DM 128.

Michael Meinzer has written a very interesting and well-crafted book. The revolutionary calendar that the convention adopted in the fall of 1793 and that lapsed in 1805 is not a historical curiosity. To the contrary, the calendar, as Meinzer rightly points out, was a structuring element in the French Republican's universalist endeavor to reshape time, space, citizen, and nation. By its nomenclatural emphasis on plant and climate, and also by its adoption of decimalization, the calendar referred the revolution back to nature and rationality, the twin abstract principles of Jacobin ideology. In the debate between those Toc-

quevilleans who see the French Revolution as institutional continuity, appearances notwithstanding, and those students of 1789 (radicals and conservatives) who see the revolution as a break, the study of Romme's calendar will definitely comfort the latter. Few precedents for calendrical reform were floated before the revolution, although decimalization was indeed presented by Condorcet (who else?) as an organizing principle of geographical subdivision. The calendar of 1793 was an emphatically novel, dechristianizing, and universalist statement. To be sure, its proponents hoped also to make it more practical and more appealing to the public, but here as in so many other ways the public of the revolution did not respond. Writ small, the calendar presents us with an interesting test case for the success of a political system based on popular sovereignty that was unable to secure a durable popular adherence.

Problems with the calendar emerged. It was supposed to mark time, but that mundane function was clouded by the urge to use the calendar for purposes of political pedagogy: "the calendar reform derives from the necessity and the possibility to cultivate a different, better person." The celebration of revolutionary fetes was a critical concern, but the public was inevitably disoriented by the rapid shifts in the choice of days that the Republic chose to celebrate (August 10? 9 Thermidor?). Typically, the Directory reasserted the use of the calendar (in the year VI especially, after the coup of Fructidor), but its aim was nonetheless more modest: it would suffice for the followers of Sieyès that the new calendar should structure daily time in a non-Christian sort of way (by allowing marriages on *décadis* only, for example.)

One of the most interesting aspects of Meinzer's book is his study of the use of the calendar in and near Marseilles. Ingeniously, he chronicles the use of the new system in hospitals, by fishermen, and by ordinary persons. He finds, surprisingly, that it was during the Consulate, when public opinion rejected the calendar, that the local bureaucracy (in Marseilles especially) was most assiduous in observing it.

In his conclusion, Meinzer observes that the calendar, although it did not succeed, did not vanish without a trace either. Christian and Napoleonic holidays soon superseded those of the revolution, but their placing in time was nonetheless made more rational. As in the United States today, for example, holidays were celebrated not on the precise day of an anniversary, but on another more convenient day close to it. Austerlitz, for example, was memorialized on the first Sunday of December rather than on December 2. Meinzer shows also that ancient clerical ukases on when marriages might or might not be celebrated lapsed during the revolution and only gradually came back to life after 1815. He concludes that, anthropologists notwithstanding, sustained bureaucratic pressures can transform customs that seem embedded in collective memory. In short, this is an

excellent book that gives a rare insight into how the universalism of the Jacobins was worked out in the field.

PATRICE HIGONNET
Harvard University

RACHEL G. FUCHS. *Poor and Pregnant in Paris: Strategies for Survival in the Nineteenth Century*. New Brunswick, N.J.: Rutgers University Press. 1992. Pp. xii, 325. Cloth \$45.00, paper \$18.00.

Rachel G. Fuchs's book moves well beyond the biological and social facts of pregnancy to provide a sensitive and multifaceted examination of the institutional, ideological, and political frameworks in which impoverished nineteenth-century French women (especially those in Paris) experienced pregnancy and childbirth. The first chapter gives a statistical overview of the "poor and pregnant" under study, based on samples of women who gave birth at *la Maternité*, Paris's major lying-in hospital for poor women from the late 1830s to the 1880s. The women sampled were generally unmarried, working as domestics or in the garment trades, and born outside the department of the Seine. Over time, samples consisted of increasing proportions of married women and those working as domestic servants. Birthplaces of migrants included areas increasingly distant from the capital, reflecting the evolution of migration streams.

After introducing the protagonists, Fuchs divides the rest of the study into several themes, which makes the study more like a set of essays on selected themes than a straightforward narrative. First, Fuchs documents changes in attitudes toward unmarried mothers. She traces the gradual decline of public arguments that viewed the single, pregnant woman as the model of moral depravity. Fuchs ascribes this change in public attitudes to a growing fear of depopulation, especially in the 1880s and 1890s. Unmarried and married women alike, she argues, gained increasing entitlements from a nascent French welfare state essentially because of public interest in the children they bore.

In chapters 5–7, Fuchs traces women through the different stages of their pregnancies, detailing the institutional supports available to them. She shows how women lived their experience—in the words of the title, what "strategies" they followed—from the time they became aware of their pregnancies until after they had decided how to deal with them. Fuchs considers both married and unmarried women, showing that women's marital state, or more importantly the actual financial support of a man, often differentiated survival strategies of the two groups. Although Fuchs occasionally harkens back to the first half of the nineteenth century for this part of the discussion, most of the analysis focuses on the period after the middle of the century. In chapter 8, on abortion, infanticide, and child abandonment, she

provides a counterpoint to these preceding chapters, showing how and why some kinds of women decided to end their pregnancies.

Fathers of illegitimate children are relatively absent from the story. Fuchs notes that her institutional sources probably understate men's supporting roles, since acknowledging their presence could sometimes jeopardize women's eligibility for assistance. She shows, however, that all-male juries were unlikely to mete out harsh punishment to women accused of abortion and infanticide, particularly those who were "seduced and abandoned." This expression of popular attitudes contrasted sharply with the bombastic bourgeois discourse that sought harsh repression of anti-natalist practices during France's *fin de siècle*.

Fuchs's book is intended as a work of feminist scholarship, and it displays clear sympathy for the women whose lives it portrays. Yet the text gains part of its strength from the author's ability to stand back and describe, often with great restraint, the miserable paucity of choices the poor and pregnant had, as well as the ideological and institutional forces with which they had to contend. Although some of the discussion of attitudes toward pregnancy in chapters 2 and 4 seems far removed from the social world, Fuchs is at her best in the more numerous chapters where she uses her rich documentation to reconstruct so vividly women's lives at various stages of the pregnancy drama.

KATHERINE A. LYNCH
Carnegie Mellon University

KAREN OFFEN. *Paul de Cassagnac and the Authoritarian Tradition in Nineteenth-Century France*. (Modern European History, France.) New York: Garland. 1991. Pp. xi, 385. \$88.00.

How is it that Karen Offen, well-known for her studies of the liberal causes of the women's movement in France, has written a biography of Paul de Cassagnac (1842–1904), a notorious authoritarian and a celebrated spokesman for the Bonapartist cause during the early Third Republic? The answer is that she has brought to fruition a project she began some twenty-five years ago as her doctoral dissertation. This final version reveals the intellectual maturity she has acquired in the intervening years. Hers is a well-researched, and, I might add, well-written study. It draws heavily on archival material, notably the Cassagnac family papers and the Bonneval-Mackau collection, the latter a gold mine for the political and financial dealings of the Right during the era of the Boulangist crisis. Offen has also written what she characterizes as a "politically incorrect" biography. She points out that "being neither French, nor a Catholic monarchist, nor a male authoritarian democrat," she had no ideological reason to view Cassagnac favorably. Yet she clearly admires the man for his energy, integrity, and especially his idealism in pur-

suing his relentless and ultimately futile battle in behalf of the Bonapartist cause.

Offen's biography of Cassagnac focuses more on his public than on his private life, but given his implacable political activism, he may not have had much time for the latter. She traces his career chronologically—from his campaign for a restored empire in the 1870s to his rear-guard opposition to the *ralliement* in the 1890s. She skillfully groups her themes as she reconstructs his life history. She highlights his background among the rural *notables* of the southwest, his early and ongoing role as a polemical journalist, his defense of the moral authority of the Catholic church in French society, his emergence to prominence as a leading Bonapartist in the Chamber of Deputies, his open quarrel with the Bonapartist faction that leaned toward republicanism, his efforts to build a broad coalition of right-wing forces to provide an authoritarian alternative to the libertarian republic (which he uncharitably labeled "*la gueuse*"), his relentless combat against the Opportunists (to whom he preferred *n'importe qui* in a motto for which he was famous), his parallel track campaign with the Boulangists to challenge the republic, and his intransigent opposition to the *ralliement* in the name of religious liberty.

Offen portrays Cassagnac the politician as a dynamic blend of old-style tactician and new-style strategist. By temperament he had the manners of a courtier; honor mattered to him, as did fair play. He could also enjoy a good fight, whether sparring in swordplay on the dueling grounds or delivering inflammatory speeches in the Chamber of Deputies. Offen suggests that his habits were rooted in a monarchist political tradition that ran parallel to the revolutionary tradition through the early nineteenth century. Both were traditions of combat that had to make some accommodation in the late nineteenth century with the new mass politics of persuasion. If Cassagnac's political values were doctrinaire, Offen observes, his political instincts were pragmatic. Indeed, he was forward-looking in moving away from the personalism of the monarchist ideal toward "solutionism," a more abstract conception of authoritarian democracy that he hoped would enable the splintered forces of the Right to regroup.

Offen takes exception to René Rémond's thesis that Bonapartism was *en bloc* a precursor of twentieth-century nationalism, and ultimately fascism. She argues persuasively that Cassagnac epitomized an important strand of Bonapartism that stood for an anachronistic political ideal grounded in monarchism, Catholicism, and the traditionalism of the rural world. One of the last of the *notables*, Cassagnac would have been uncomfortable with the more urban, plebeian nationalism that emerged to prominence at the turn of the century. She aptly characterizes Cassagnac's relationship to the new nationalists as that of "unfriendly cousins within a larger clan." They may have been successive representatives of an

ongoing authoritarian tradition, but they ought not to be conflated in a facile way. Indeed, she has rescued Cassagnac from his reputation as an eccentric by the clear and discerning way in which she relates his lifework to larger political currents. Her biography helps us to understand the man with more sympathy and the political world in which he moved with greater nuance. In this genre, hers ranks with the best of them, *n'importe qui!*

PATRICK H. HUTTON
University of Vermont

MICHAEL MARLAIS. *Conservative Echoes in Fin-De-Siècle Parisian Art Criticism*. University Park: Pennsylvania State University Press. 1992. Pp. x, 245. \$35.00.

The late Herschell Chipp, in his lectures on the art of the *fin-de-siècle* at the University of California at Berkeley, used to state with great aplomb that it was an era in which artists found themselves either at the foot of a cross or the end of a gun. Reasons for this crisis in the world of art were delineated in his ever-popular textbook, *Theories of Modern Art: A Source Book by Artists and Critics* (1968). In that work the tortured thoughts of Vincent Van Gogh, Paul Gauguin, and Edvard Munch, among others, were deftly excised from letters and other source material to illustrate the subjectivist tendencies found in art at the end of the nineteenth century. Now, nearly twenty-five years later, a book has appeared that evaluates the crisis from the written opinions of art theorists themselves who were working in and around Paris during the last two decades of the nineteenth century.

Michael Marlais has written an important and compelling work that traces the intellectual reasons behind the rise of antinaturalism, idealism, and symbolism in the art capital of the world before 1900. As the revolutionary technique of Impressionism began to grow old, the desire for more thought and less sensory input in art emerged. Ironically, this desire tended to unite conservatives, who had railed against Impressionism in the first place, with avant-gardists who felt that the movement had become old-fashioned, meaningless, and that photography had made naturalism in painting irrelevant. Suddenly, Catholic revivalists seeking a return to ideals in art, politics, and morals, found themselves allied with the avant-garde in their hatred of the French scientific/positivist establishment. As Marlais succinctly says, "Given the rationalist, scientific basis of much in French thought until this time, it now became, paradoxically, modern to be reactionary" (p. 18).

It was a testimony to the power of positivism, which had so permeated the Academy and other French institutions, that it was able to bring to a united front the opposition of such disparate groups as Catholic polemicists, anarchists, royalists, and nationalists. It was a time when both the far Left and the far Right

looked on the promises made by science and democracy with growing skepticism. It was a time when the mere execution of a brush stroke held philosophical, religious, and political ramifications.

The passion and brilliance that theorists like Albert Aurier, Félix Fénéon, J.-K. Huysmans, and Maurice Denis brought to their discussions of art make Marlais's book a pleasure to read. For instance, when the symbolist Aurier attacked science for "asserting that for its lenses and scalpels there did not exist a single mystery," he concluded his diatribe with all the flourish of a poet, claiming that scientists themselves "have nothing to put on old Olympus, from which they have removed the deities and unhinged the constellations" (p. 69).

Recognizing that this is a book in which ideas hold the spotlight, it is nevertheless a small criticism to say that in the extraordinary breadth of his analysis, Marlais has been forced to sacrifice some depth. For instance, the colorful Joséphin Péladan, a Catholic triumphalist and self-proclaimed "Sâr" (the title given a Chaldean magus), graces the cover of the book in portrait, but there is little discussion of his life or the esoteric Salon Rose + Croix, which he initiated. That said, this book's primary value can be found in the skillful presentation of the intellectual ideas that helped shape a crucial—but little understood—phase in the development of modern art. It is a credit to the author that these ideas seem as fresh and relevant to the current debate over art, politics, and morality, as they did a century ago.

MICHAEL MORRIS

St. Mary's College of California

HERMAN LEBOVICS. *True France: The Wars over Cultural Identity, 1900–1945*. (The Wilder House Series in Politics, History, and Culture.) Ithaca: Cornell University Press. 1992. Pp. xix, 221. \$29.95.

After the outpouring of analysis that accompanied the bicentennial of the French Revolution, this innovative book examines one of the offspring of that "mother of all revolutions," nationalism. It is not, however, about the nineteenth-century variation that espoused the self-determination of all peoples, but rather the more nefarious twentieth-century variety that took shape in France during the Dreyfus affair.

Herman Lebovics, whose previous book, *The Alliance of Iron and Wheat in the Third French Republic, 1860–1914* (1988), dealt with the new conservatism of the Third Republic, attempts here to look at modern French nationalism from a different perspective. He brackets his 1900–45 time period with biographical studies of two representatives of anthropological and ethnographic science: Louis Marin, an influential conservative politician who began teaching at the École d'anthropologie in 1895 and presided over it in the 1930s; and Georges-Henri Rivière, who founded the Musée d'Arts et Traditions Populaires in the late

1930s. In between Lebovics examines the Colonial Exposition of 1931 at the Bois de Vincennes, French colonial education policy as practiced in Vietnam, and the creation of the Musée de l'Homme by Paul Rivet during the era of the Popular Front.

It requires something of a tour de force to make these disparate elements cohere into a framework to analyze the "wars over cultural identity" advertised by the subtitle of the book. Lebovics posits three models of the "True France" to help mark the way. One he calls the conservative "essentialist" vision of a rural, traditional France, in danger of disappearing due to corrupting modern and foreign influences. The second he describes as a left-wing variation on this theme, more republican, urban, and inclusive of workers, but similar in its assumption of an "essential" French nature that all in the country were supposed to share (or, for outsiders, assimilate to). The third France, which Lebovics unabashedly advocates and which he claims Rivière first set forth, is a cosmopolitan (Lebovics judiciously avoids using the term multicultural) antithesis of the essentialist identities that recognizes cultural identity as a complex amalgamation of cultures from different regions of France as well as colonial and foreign people who have come to reside there.

This is not a tightly organized, highly polished treatise on French nationalism. But it holds up better than the study of "the confluence of discursive formations, or ideologies, and power-saturated practices at different moments in different settings" (p. xiv), which is all that Lebovics promises at the outset. Moreover, in addition to using archival materials on colonial expositions and administration, anthropological museums, and the private papers of Marin and Rivière, he presents an excellent bibliographic synthesis of the latest writings about cultural theory and on education and the social sciences in twentieth-century France.

Lebovics's book is on the right subject at the right time. He seeks above all to place today's surprisingly resilient nationalism in a fresh historical perspective. Despite some detours along the way, he succeeds admirably in reaching his goal.

WILLIAM H. SCHNEIDER

*Indiana University–Purdue University,
Indianapolis*

MICHÈLE C. CONE. *Artists under Vichy: A Case of Prejudice and Persecution*. Princeton: Princeton University Press. 1992. Pp. xxiv, 264. \$35.00.

Far from stultifying or withering completely, the world of art in occupied France sprang back immediately, flourishing with unabated intensity and generous support. The kind of art that Vichy promoted—beyond the traditionalism emphasized in previous studies—and the response of artists to the political exigencies is the subject of Michèle C. Cone's book.

There is, quite clearly, much new and valuable information contained in the book, much of it surprising even for those who are familiar with the occupation period. But particularly revealing is her treatment of what the sociologist Pierre Bourdieu termed "the restructuring of the artistic field," or the shifts in the internal hierarchy of power. Not only did Vichy undermine the extant official hierarchy in art but also its spokesmen in the press were unremittingly critical of the Academy and the Salon. They equated academicism with compromise and the "Jewish style," perhaps because many of the artists admired the "older" nationalist Right of the Action Française. Those in the strongest positions in the Vichy period were those whom Cone describes as occupying "the middle of the road"—the "Romantic Realists"—whose ideology was extremely apt.

Vichy, in addition, favored and promoted other kinds of art; the National Revolution, like the French Revolution, sought to "imprint society with artistic imagery." Hence it stressed the decorative arts and the importance of creating cultural objects specifically designed for each of the classes, from the popular to the elite. In decorative arts even the Cubist style could be acceptable, although generally the regime favored a naïveté that signified the birth of a mythic "new France." Moreover, Vichy officials preferred to view the artist as an artisan, collaborating with others in the creation of beautiful, useful, and saleable objects. As Cone observes, this stress explains the fact that Vichy recognized no one group of artists as the regime's official image makers.

But Vichy did promote certain themes as well as selected traditions and techniques designed to incarnate the cultural and political values of this "new France": for example, "pure" or primary colors as well as a consciously naïve style carried connotations of the vaunted political and cultural rebirth. And prominent critics like Lucien Rebatet promoted the example of the Barbizon school as being "safely premodernist in outlook without being pompier." Cone also notes the relative leniency of the German censors who surprisingly tolerated the Fauves, now interpreted by critics as embodying a "dynamic fascist force against world decadence."

Equally absorbing is the book's treatment of the various tactics artists employed to communicate a message other than the one desired by Vichy officials. The Surrealists had particularly ingenious tactics, which included occultism, as did other groups within the wide-ranging resistance movement. Resistance, as Cone aptly observes, fostered a variety of responses ranging from extremes of hermeticism and escapism to a blatant socialist realism. And it too had its codes just as in officially recognized art, and they were also often indirectly expressed by techniques such as choice of colors. Images, under the circumstances, were devised to serve multiple ideological ends depending on the interpretive techniques employed by the observer.

Particularly intriguing here is Cone's discussion of Picasso who, although a celebrity, as a foreigner was still in a precarious situation: considered a "decadent" because of his style, he was prohibited from exhibiting, but he nevertheless worked prolifically, communicating his resistance and personal response through both imagery and style. But equally engrossing is the treatment of artists like Francis Picabia, whose case raises the knotty question of what exactly complicity or collaboration comprised. As Cone demonstrates, this was to cause many problems in the period after the war, when artists formed purge committees, one of which was led by Picasso.

Perhaps the major weakness of the book is its narrowness of context, its avoidance of larger relevant issues and phenomena outside the artistic world. Not only does it generally ignore the inclusive discursive context but also it pays little attention to the prior evolution of the concepts and terms invoked. Moreover, Cone often cites journals but provides scant information on their nature or status; for example, she identifies the profascist *Je suis partout* only as a "right-wing journal." Brasillach receives only passing mention and we are not told that Lucien Rebatet was also a prominent critic of music, a fact that explains many of his terms and metaphors. Although the discussion of his rhetorical tactics is strong, we are told summarily that he was probably a Nazi "plant," but given no other information or verification.

In addition, many of the tendencies, terms, and themes the book refers to are familiar to those who have studied the arts in France preceding the war. The values associated with "Les primitifs" (referring to Romanesque and Gothic art) date back to the period before World War I, in artists like Maurice Denis; similarly, the perception that the roots of modern French art could be traced to the Romantic period was developed by prefascist thinkers in journals like *l'Indépendance*. By tracing such a discourse one could argue that the Vichy mentality and aesthetic was prepared in the cultural discourses of specific political groups in the decades preceding the war. This kind of approach would seem to provide the kind of explanatory context that Cone avoids but which is so essential to understanding what transpired.

Cone's approach as well as her distinctive points of emphasis are explained most fully in the epilogue, where her primary motivation emerges. Here she openly poses the question of why French artists "compromised themselves," a question implicit in the book's subtitle. This also explains otherwise extraneous discussions such as her extensive treatment of the harsh conditions in the internment camps, well documented in other sources; it similarly explains omissions that could have been equally illuminating, such as the bureaucratic reorganization of the art world during this period. The book, while valuable as an introduction to the basic issues, would surely have

benefited from a far more rigorous or systematic historiographic approach.

JANE FULCHER
Indiana University,
Bloomington

GRAHAM KELSEY. *Anarchosyndicalism, Libertarian Communism and the State: The CNT in Zaragoza and Aragon, 1930–1937*. (Studies in Social History, number 11.) Boston: Kluwer Academic and International Institute of Social History, Amsterdam. 1991. Pp. ix, 312.

The Spanish anarchosyndicalist trade-union organization *Confederación Nacional del Trabajo* (CNT), along with its anarchist ally, the *Federación Anarquista Ibérica* and the Trotskyist *Partido Obrero de Unificación Marxista* (POUM)—all long topics of fascination to political historians but seldom written about—made up the libertarian Left during the Spanish Second Republic and subsequent Civil War. During the first ten months of war, these three groups organized the northeastern regions of Republican territory through the Anti-Fascist Militia Committee of Barcelona, the Council of Aragon, and the militia army. They were opposed not only by the Nationalists of General Francisco Franco but also, quietly, by the Spanish Communist Party (PCE) and the *Partit Socialista Unificat de Catalunya* (PSUC). After the May crisis of 1937 in Barcelona (the backdrop of George Orwell's *Homage to Catalonia*) Soviet aid to the Republic enabled the communists to gain power and then control. The resignation of premier Francisco Largo Caballero on May 15, 1937, led to the abolition of the council and the committee and the persecution of the POUM, signaling the communist domination of the Republic until late February 1939, only a few weeks before its surrender.

There are few scholarly works on the CNT, and fewer still in English. The late Burnett Bolloten's *The Grand Camouflage* (1961) and his revised *The Spanish Revolution* (1979) bear on the subject, but both are much wider in scope. So Graham Kelsey's study of the CNT in Zaragoza and Aragon is an important contribution, all the more so because he focuses on the CNT not in Catalonia, as I did in an early work, but in Aragon, little studied in the civil war literature.

Zaragoza, Kelsey stresses, was a labor union city often in turmoil where strikes were usually put down with ruthless violence by the Civil Guard, a tradition that continued during the early months of the Second Republic in 1931. The conjunction of economic recession, Republican desire for law and order, and a failure to root out corrupt and conservative political bosses in Aragon created immediate tension with the anarchosyndicalist unions. Kelsey's picture of Zaragoza is extremely valuable and carefully done.

The CNT in Aragon was caught between its own wings of pure anarchism and a syndicalist labor movement. Kelsey argues that, as government pres-

sure increased, both wings fused into militant trade-union action far more than in neighboring Catalonia. Uprisings in December 1933 and April 1934, however, were repressed, keeping Aragonese labor out of Red October 1934 (the Asturian revolution) and weakening the CNT militants at the outbreak of the Civil War in July 1936. As a result, they seized only a few key positions in the center of the city, an extraordinary failure for the most militant section of Spanish anarchism. The author's explanation of this situation is plausible and logical, as is his discussion of the subsequent development of the libertarian, collectivized countryside. The subsequent Aragon campaign, led by Buenaventura Durruti, is not included, since it had Catalan rather than Aragonese roots.

In all respects, this work is a valuable contribution. The writing is good, and, although it covers only one aspect of political development during the Second Republic and the Spanish Civil War, it is an extraordinarily clear, focused monograph, the best recent book on the period and the best on anarchism.

ROBERT KERN
University of New Mexico

KARIN TILMANS. *Historiography and Humanism in Holland in the Age of Erasmus: Aurelius and the Divisiechroniek of 1517*. (Humanistica and Reformatorica, number 51.) Nieuwkoop: De Graaf *Divisiechroniek*. 1992. Pp. xi, 409. f. 125.00.

Cornelius Aurelius (1460?–1531) was an Augustinian canon associated with houses of the Windesheim Congregation. His *Divisiechroniek* is a vernacular history of Holland divided into three historical periods, beginning with the creation. He was also a correspondent of the young Erasmus, a publicist, and a member of the delegation sent to reform the Abbey of St. Victor in Paris. In spite of his connections, he remained a secondary figure.

The title of Karin Tilman's study, a translation of a dissertation published in Dutch in 1988, raises expectations that are not quite fulfilled. The epitaph by Aurelius's friend Alardus significantly concludes with the line *Quod veri monachi vera erat effigies* ("he was truly a perfect monk," p. 325); much the same could be said of the medievalism of his *Kroniek*. Its humanism consists primarily of additions from easily available classical authors as well as biblical and medieval sources (tables on pp. 125, 175, 179–81) that he uses rather uncritically; some examples of his borrowings are given in the appendixes. Aurelius's aim was to teach the difference between good and evil and to that purpose he also uses the miracles of saints. Aware of controversies in his sources he glosses them with "we do not want to deal with these" or "for the sake of brevity [I leave] them out" (pp. 117–18). At one point Tilmans cites in apparent agreement Jan Romein's view of the *Kroniek* as "purely medieval in character" (p. 128), although this view is at odds with

Tilmans's earlier assertion that Aurelius adopted a "programme for historical writing which one could describe as being essentially humanist" (p. 40). Tilmans's final assessment (pp. 326–29) of Aurelius remains ambivalent.

Tilmans devotes some ninety pages to a discussion of the antiquarian problem of the origin and location of Batavia, which had occupied the energies of local erudites and was the subject of two smaller works by Aurelius. In these works Aurelius showed himself aware of the fact that classical texts were corrupted by later glosses—"Caesar is not always Caesar" (p. 229)—but he also maintained that Holland was already inhabited at the time of Isaac and that "the Trojans arrived during the time of Samson" (pp. 208–09), a fair example of Aurelius's approach to the legendary past.

The text is also not always clear. What are we to make of: "this concept of *dulce solum natale* applied to all Holland (i.e. Batavia) in contrast to Johannes a Leydis' idea of *patria* which was used in the much more general sense of country or region of origin" (p. 162–63)? "Batavia" was only a part of Holland, and even if we make the county of Holland into a "country," how is it different from Leydis's concept? I suspect that something got lost in translation. The translation also sometimes alters the sense of the original; "Wi souden van den sommigen die noch leven grote indignacie ende onwaerdicheit verwerven ende gecrigen" becomes "We would bring indignity and dishonour to some of those still living" (p. 128) instead of "We would earn and receive [be subjected to] the indignation and contempt from those still living." Minor problems occur also: Tilmans describes Aurelius's style as "verbose rhetoric" and supports it with another's assessment of it as "*prose savoureuse*" (p. 138, n. 82); members of the *Hof van Holland* (a council and legal court) who are either councilors or advocates occasionally are called "courtiers" (as on p. 75); and the Abbey of St. Victor in Paris is given a reputation for "scientific learning" (p. 31, for *wetenschap*?) where "scholarship" would be more appropriate.

DERK VISSER
Ursinus College

PETER ARONSSON. *Bönder gör politik: Det lokala självstyret som social arena i tre smålandssocknar, 1680–1850* [Peasants Making Politics: Local Self-Government as a Social Arena in Three Småland Parishes, 1680–1850]. (Bibliotheca Historica Lundensis, number 72.) Summary in English. Lund, Sweden: Lund University Press. 1992. Pp. 380.

According to many accounts, Sweden became a modern, democratic welfare state almost overnight. Not so, argues Peter Aronsson, who maintains that, even as far back as the 1680s, Swedes enjoyed a significant degree of self-government, albeit at the local level.

Aronsson draws his evidence for self-government mainly from records of three Småland parishes of the seventeenth, eighteenth, and nineteenth centuries. Parish protocols and other records tell of the functions assumed by local officials, the disputes to which conflicting interests gave rise, and the types of arguments used to shape decisions. They reveal that the freeholding peasants—the *bönder*—were neither inactive nor mute. They came to meetings, protested taxes and other impositions, quarreled, voted, and forced officials to heed them. Aronsson gives scores of examples.

Two things call for explanation. One is why the peasants had this influence, and another is why people today should find it surprising. In regard to the second, Aronsson says that earlier Swedish historians simply overlooked the peasants. They wrote history with an upper-class bias, assigning "uncultured" peasants a negligible role. Modern social scientists, in contrast, have no difficulty recognizing that peasants have their own folk culture. A modern historian grasps what was unthinkable for earlier historians: that peasants are quite capable of recognizing a common interest and acting on it.

As to why there was such a scope for peasant influence, Aronsson writes that local government was largely a creature of a church that inadvertently promoted popular participation. The church from its inception in thinly populated Sweden cultivated the good will of local people to build and protect its widely scattered sanctuaries. The church's parish priests were assigned only with the consent of local congregations. When the Reformation deprived the church of much of its property, it became even more dependent on local good will for its upkeep. That gave the clergy even more reason to consult and placate peasants. Aronsson's reading of the records gives many instances of priests despairing about the casual manner in which wayward peasants treated the church—not attending services, loitering around the grounds, coming drunk, arriving late, playing cards, leaving early, and so on—yet feeling it inadvisable to do anything except scold them for impiety. Priests did not rely on state coercion. Moreover, the religious faith urged by the church cut both ways. The peasants learned to use it to uphold their own interests, calling on the clerics and other state officials to show the same concern for divine justice that they preached to others.

The description of peasant participation in government is enhanced by an overview of the social classes, including the different grades of peasants. Actions taken by the parish councils are summarized in tables. Beyond giving a highly informative account of rural self-government, Aronsson makes two claims for his work. He says it shows that local histories can correct impressions generated by more conventional histories that overplay the role of national elites. And he says it shows that modern Swedish institutions of self-gov-

ernment have historical antecedents. A seven-page summary in English is appended.

BERTIL L. HANSON
Oklahoma State University

EVA ÖSTERBERG. *Mentalities and Other Realities: Essays in Medieval and Early Modern Scandinavian History*. (Lund Studies in International History, number 28.) Lund, Sweden: Lund University Press. 1991. Pp. 207.

This volume is a collection of translations of essays that were published in Swedish between 1979 and 1991. Their primary subject matter is the social relations and political culture of the Swedish peasantry of the preindustrial period, although some complementary work dealing with medieval Scandinavia is also included.

Eva Österberg is well acquainted with the work of English and European social historians and is obviously trying to fuse traditional Swedish social history with perspectives drawn from the *Annalists*. The results are illuminatingly mixed: Österberg amply succeeds in demonstrating how much more remains to be done in the field of Swedish history "from below," but her work also illustrates how difficult this project is, for primary historical documents are exceedingly scarce for this period. Österberg's analysis is frequently hampered because the records of the localities under study do not overlap each other chronologically, thus preventing her from making synchronic comparisons. It is also clear that her approach must be a novel one in Sweden, for much of her writing is devoted to explaining the *Annaliste* point of view and methodology, as well as reviewing the non-Scandinavian work (usually in English) that her own studies use as a model and starting point. The articles that comprise chapters 2 ("Finance and Fun: Changes in Historical Research"), 4 ("Historical Perspectives on Laughter and Play"), and 7 ("The Civilizing of Swedish Peasant Society in the Seventeenth Century") are scarcely more than meditations on their subjects, but presumably they were suggestive enough in Sweden to merit publication.

Österberg's talents naturally appear at their best when they are turned to subjects that are relatively richly documented. Chapter 3, "Witnesses to Wonders: Miracles as Evidence for Medieval Mentalities," is an interesting demographic and thematic analysis of the records of the fifteenth-century courts investigating the possibility of canonizing Bishop Nils of Linköping and Katarina of Vadstena. Chapter 5, "The Good Old Times: Contrasting Pictures of Swedish Peasant Society," compares property sales and sexual offenses in seventeenth-century England and Sweden. Chapter 6, "Violence among Peasants: Comparative Perspectives on Sixteenth- and Seventeenth-Century Sweden," investigates the criminality of the period, again in the context of English studies of the same subject. In all of these essays, Österberg engages

in a critical dialogue with other Scandinavian historians as well as with her evidence.

Some of the other articles are less convincing. Chapter 8, "Change and Adaptation: Mechanisms of a Peasant Economy," reviews the normative model proposed by Kåre Lunden in 1974, only to conclude that it is too simplistic—by no means an original insight. Chapter 9, "Peasant Upheavals, Economy, and Ownership in Medieval and Early Modern Scandinavia," tests various theories of peasant revolt against the Scandinavian data. Österberg raises important questions, but, as usual, not enough evidence is available to suggest even hypothetical answers. Moreover, her methodology raises several questions of its own. Why assume that all peasant revolts should be explicable by a single theory? If none of the previously proposed theories sufficiently accounts for the Scandinavian revolts, why not put forward one that does? Chapter 10, "Alternative Protests among Ordinary People in Early Modern Sweden," suffers from the gratuitous use of fashionable concepts. It asks why peasants did not object to the growth of absolutism in seventeenth-century Sweden, and answers that centralization and bureaucratization did not oppress the peasantry further and may even have provided greater access to the central government. Yet having shown the possible advantages of absolutism, Österberg inconsistently tries to depict peasant passivity of the period as a form of "alternative protest." Similarly, chapter 11, "Local Political Culture versus the State: Patterns of Interaction in Pre-Industrial Sweden," tries to add a veneer of sophistication by assuming that "people's strategies for the future are also shaped by their memories of the past, their perception of the outcome of previous confrontations with authority" (p. 189). Applying this assumption yields such ambiguous results that our understanding of the problem is not at all improved.

The shortcomings of these studies attest more to the lack of relevant data than to any failing in Österberg herself. Unless more supplementary information can be gleaned from the literature and private records of the time, even the most brilliant historian would be forced to conclude with her that the paucity of the evidence shows us the limits of our knowledge, if nothing else.

ELIZABETH ASHMAN ROWE
Stanford University

DAG LINDSTRÖM. *Skrå, stad och stat: Stockholm, Malmö och Bergen ca. 1350–1622* [Guilds, Cities, and the State: Stockholm, Malmö, and Bergen, ca. 1350–1622]. Summary in German. (Studia Historica Upsaliensia, number 163.) Stockholm: Almqvist and Wiksell International. 1991. Pp. 303. 176 KR.

Dag Lindström's book is a detailed and systematic study of the legal status and functional roles of craft guilds in three Scandinavian towns from their first

documented appearance in the late Middle Ages to the introduction of kingdom-wide reforms in 1621 and 1622. Chosen for the study were one town in each of the Scandinavian kingdoms, namely, Bergen (Norway), Malmö (Denmark), and Stockholm (Sweden). As relations among craft guilds, town councils, and royal governments are of central importance to the study, it must be noted that Bergen was under Norwegian sovereignty for only the first twenty years of the period, thereafter finding itself under the rule of Danish monarchs who, from the mid-1530s, viewed Norway as a mere province. Further complicating the special case of Bergen is the fact that the Hanseatic League's extraordinary privileges and presence in the town continually posed problems for the exercise of royal power there until the late 1550s.

The first of Lindström's eight chapters is a brief introduction to the problematic with which he deals, and the last presents a summary of his findings. In chapter 2 he reviews the general literature on craft guilds and then tackles the concept of "state," the nature of the feudal state and the absolutist state, and general political developments in Scandinavia during the period. In chapter 3 the author compares the population and economies of the three towns, the nature of their town governments, the presence or absence of royal power, and the special situation of the privileged German population in Bergen. Only in chapter 4 does Lindström begin his examination of the craft guilds themselves, following that overview with chapters on the situation of these guilds in relation to town and royal governments, control over production, and the introduction of guild reforms in 1621 and 1622.

The heart of this study is its treatment of power relationships and functional relationships among craft guilds, town councils, and royal governments, a focus distinguishing Lindström's book from earlier studies of Scandinavian craft guilds. These relationships shifted over time, of course, just as they differed from town to town and kingdom to kingdom synchronically. Lindström successfully demonstrates that the structuring of craft production through the guild system provided mutual benefits for both guilds and the governmental bodies to which they answered during the Middle Ages. As royal power was consolidated in the rival kingdoms of Denmark-Norway and Sweden during the sixteenth and early seventeenth centuries, influence over the guilds shifted markedly from town councils to royal chanceries. Both monarchies strove to promote the crafts during this period—"probably in order to supervise, utilize, and tax" them (p. 216)—but they adopted markedly different strategies. Although the Swedish crown "systematically helped the craft guilds and promoted an expansion of the guild system" in its efforts to promote the crafts, the Danish crown frequently pursued the same goal by choosing "to minimize the significance of the

guilds and to promote urban crafts outside of the guild system" (p. 216).

MICHAEL F. METCALF
University of Minnesota,
Twin Cities

FREDERIK OHLES. *Germany's Rude Awakening: Censorship in the Land of the Brothers Grimm*. Kent, Ohio: Kent State University Press. 1992. Pp. x, 227. \$35.00.

Censorship in early nineteenth-century Germany has attracted scant attention among English-speaking historians. Given the region's political fragmentation, the best approach is undoubtedly that adopted by Frederik Ohles, that is, to investigate thoroughly all aspects of censorship that can be documented in one of Germany's smaller states. Although "atypical" (which German state was not?), Hesse-Cassel (following Ohles's spelling) was a good choice. Also known as Electoral Hesse, the principality was poor and overwhelmingly agricultural, with a population of some 600,000 in the 1820s.

This was a period of rapidly changing print technology, growing literacy, and expanding public opinion (the "rude awakening"). Following Robert Darn-ton, Ohles wants to "keep one eye on authors, publishers, printers, shippers, sellers, and readers . . . and the other eye on governors, jurists, administrators, police, and censors" (p. 4). Inevitably, by using sources generated primarily by state officials, the focus is more on the latter than the former.

After explaining the "challenge of censorship," Ohles details Hesse-Cassel's general history, especially the celebrated cause of Sylvester Jordan, Marburg jurist and primary author of the liberal constitution of 1831 who suffered imprisonment under later repression. Then he launches into various themes: censorship laws, the censors and their work, types of censorship and changes in the practice of censorship, and so forth before taking up "Friends of a Free Press," in the penultimate chapter. The concluding chapter explains the "Failure of Censorship."

What strikes the reader is the uncoordinated, slipshod nature of the effort to impose censorship. The Elector's Censorship Commission of 1816 had three (there were never more) part-time censors, one of whom for a time was Jakob Grimm, the philologist. Underpaid, ill-supervised, following unclear instructions, and even in disagreement with the intent of censorship, the censors' lot was not a happy one. "As much as anyone censors hated censorship" (p. 48), for there were so many bad books to plough through. As the number of lending libraries grew, the government wanted their holdings censored, but the censors balked just as they opposed examining the rising tide of periodicals. Into the breach came unpaid local notables or local police, all with limited authority and acting inconsistently over space and time.

The German Confederation passed legislation to

control the press but provided no effective enforcement. Rulers jealous of their state's rights extended one another little cooperation.

And what of the booksellers? That trade was not for the timid; the confiscation of banned material was never ruinous, but it was more than an inconvenience. Although booksellers, readers, and constitutionalists shared a common interest and forum, few made a principled stand leading to imprisonment. Ohles is unable to make clear to what extent publishers and booksellers preemptively censored themselves; surely this kept a heavy lid on the world of print.

As in other matters, the Confederation was ineffective and the censors never figured out what they should be doing. Nonetheless, in the 1840s tensions between governments and a formative but restive political class turned censorship from an accepted institution into a political issue. Freedom of the press and the abolition of censorship were key demands in 1848.

Supported by extensive material from the Hessian State Archive at Marburg, Ohles's sensibly brief text gives us the detail we need to reach the same conclusion as he: "the more that censors banned, the more that censorship failed" (p. 169), thus confirming conventional wisdom. An appendix lists banned publications between 1831 and 1848.

LOYD E. LEE
State University College of New York,
New Paltz

FREDERICK GREGORY. *Nature Lost? Natural Science and the German Theological Traditions of the Nineteenth Century*. Cambridge: Harvard University Press. 1992. Pp. viii, 341. \$39.95.

The triumphs of natural science in the nineteenth century forced theologians to redefine the place of nature in religion. Particularly in the wake of the Darwinian revolution, theologians could no longer take for granted that the meaning of human destiny was located in a divinely directed nature. This potential "loss of nature" is the focus of Frederick Gregory's sophisticated study of the relationship between faith and science in nineteenth-century German Protestant theology.

Rejecting Marxist interpretations as misleading and nonfalsifiable, Gregory concentrates on internal developments within philosophy and theology. Professional theologians, he argues, staked out four basic positions in response to the challenge of natural science: the Hegelian speculative, the orthodox, the mediating, and the neo-Kantian. In the book, these schools are represented by David Friedrich Strauss, Otto Zöckler, Rudolf Schmid, and Wilhelm Herrmann respectively. After a general overview of German Protestant theology in the nineteenth century, Gregory takes a biographical approach. He examines

in detail the careers and writings of these four thinkers, with careful attention to their conceptions of truth. Some readers will miss an overarching theoretical structure here, but Gregory's approach does bring the ideas to life and reveal the role of personal connections in the development of intellectual traditions.

Strauss, Zöckler, and Schmid all managed to retain nature for theology. Strauss's "new faith" effectively substituted worship of Darwinian nature for the "old faith" of Christianity; for him nature virtually absorbed theology. Zöckler's orthodoxy was a form of what we would now call creationism. Denying the dysteleology and materialism of Darwinism, he maintained that the Bible presented a true account of divine creation; for him nature had to accommodate theology. As a mediator, Schmid reconciled Darwinism and Christianity, insisting that evolution could proceed by design and was not in conflict with a nonliteral interpretation of the Bible; for him nature was in harmony with theology.

Strauss, Zöckler, and Schmid all assumed that scientific knowledge corresponded to an ontologically real nature. It was Herrmann, a follower of Albrecht Ritschl, who lost nature by rejecting the correspondence theory of truth in favor of a coherence theory. As a neo-Kantian, Herrmann severed faith from a scientific knowledge of the world (*Welterkennen*). Thus, nature was lost not in a battle between theology and science, but by the very denial that there could be such a battle. In the end, it was Kant, not Darwin, who lost nature.

No brief summary can do justice to this skillful exercise in the history of ideas. Yet Gregory's decision to restrict himself to four thinkers leaves the reader wondering how representative these men were. For whom was nature lost? "The theologians discussed in this study were spokesmen for large groups of people" (p. 4), he writes, but his method does not permit him to support this claim. Strauss was widely known, but Zöckler, Schmid, and Herrmann were not. Gregory concedes as much when he states in the epilogue that academics such as Herrmann "did not speak in a language that was understandable to most people" (p. 262). Indeed, he concludes: "Most people of faith living at the end of the twentieth century have not lost nature" (pp. 262–63). Still, he insists that the loss of nature is not of "merely academic or scholarly interest" (p. 263), because it has left religion ill-equipped to confront the ecological crisis. This is a provocative conclusion, but it would be more convincing had he paid more attention to the social context of ideas.

The book could have used more editing. The writing is often repetitive, and the organization is cumbersome and at times confusing. Nonetheless, it is a valuable book with new perspectives in some neglected areas of intellectual history. The patient reader will be well rewarded.

ALFRED KELLY
Hamilton College

ANNETTE WITTKAU. *Historismus: Zur Geschichte des Begriffs und des Problems*. (Sammlung Vandenhoeck.) Göttingen: Vandenhoeck und Ruprecht. 1992. Pp. 237. DM 29.80.

Historicism remains a notoriously manifold and nebulous notion that has been subjected to intensive dissection. The "old" historicism has been difficult enough to fathom, yet now we are said to be in the midst of a "new historicism" in literary and cultural studies. There persists the antithesis of "bad historicism" (engendering all-consuming relativism and skepticism) as opposed to "good historicism" (inducing self-critical reflection and an expanded perspective on human affairs). What has historicism become over time and what have been its major implications? Is it a form of knowledge or the "end" of knowledge? Can historicism become a world view or tradition in itself, or is it rather the most potent solvent of all world views and traditions, if not of knowledge itself?

Annette Wittkau has provided an insightful and balanced *Begriffsgeschichte* of the evolution of German historicism from roughly the mid-nineteenth century to the mid-twentieth century. The original and illuminating core of the book is its concerted analysis of disciplines other than history itself: economics, law, theology, and philosophy. Making claims to normative validity or dogmatic truth, these disciplines have disclosed historicism's most radical proclivities toward enervating relativism of values and norms, causing a gulf between the claims of knowledge and practical, existential "life," and even the nullifying of knowledge itself. As Emil Lask once observed: "Historicism . . . is the most modern, most wide-spread, and most dangerous form of relativism, the levelling of all values" (quoted on p. 113).

The work has three well-articulated major sections: the first traces the proximate origins in Johann Droysen, Jacob Burckhardt, and Friedrich Nietzsche; the second addresses its elaboration and reception in Wilhelm Dilthey, Gustav Schmoller, Emil Lask, Rudolf Stammeler, and others; the third lays forth the opposing attempts by Max Weber and Ernst Troeltsch to "overcome" its perplexities. Wittkau argues that successive crises of the social order (both sociopolitical upheavals and industrial revolutions), filtered through a slackening idealist tradition, engendered a distinctive German historical consciousness. By the turn of the century this historical consciousness had developed its own immanent crisis, which in turn signaled a crisis of the wider culture. The interaction produced a vertigo of values.

The author argues that the full dimensions of German historicism were disclosed by 1900 and continued to be vehemently debated until 1930, after which they were almost perversely obscured. The "arbitrary" works of Karl Heussi and Friedrich Meinecke served to sequester the most crucial aspects of historicism. Meinecke wound up idealizing and "Germanizing" historicism, divesting it of its urgent exis-

tential and normative implications. After 1945 historicism was largely rejected as part and parcel of miscreant German trends—the intellectual unction of the fateful *Sonderweg*.

Wittkau's work is remarkably trenchant for such a convoluted subject. She is adept at aligning camps and positions in the protracted polemics over historicism, discerning three fundamental positions: the Nietzsche-Dilthey-Heidegger line, which posited the fundamental primacy of human existence (and its historicity) over all knowledge; the more Hegelian line of Troeltsch-Scheler-Mannheim, which projected a higher synthesis of reality and methodical knowing; and the more Kantian line of Lask-Weber-Jaspers, which posed unbridgeable chasms between scientific knowledge, normative valuation, and "real" reality. Wittkau seems to favor the latter position without actually saying so.

The work is a revised dissertation and indulges in too many rudimentary repetitions, primer-like reminders, and interim summaries. For a work about vital cultural values in an age of crisis, it is at times pedestrian in tone. Wittkau's attempts to bring in actual historical circumstances, such as the shocks of World War I and the Weimar crises, seem cursory, even perfunctory, in a way that detracts from her case. Wittkau copiously employs the term "life-world" (*Lebenswelt*) but does not say what it means; similarly, she fails to delve into important concurrent strands of *Lebensphilosophie* and phenomenology. She insists on "*Kulturwissenschaften*" instead of "*Geisteswissenschaften*," but, astonishingly, the major thinkers Wilhelm Windelband and Heinrich Rickert, who argued for just this usage, are to be found only in a footnote on Lask. Neo-Kantian and neo-Hegelian revivals are hardly alluded to. The mid-range framework of historicism is slighted.

Some of her judgments about individuals seem flawed or elliptical. Nietzsche and Dilthey are linked together as insisting on the primacy of "life" over historical knowledge, but the author fails to note Dilthey's censure of Nietzsche's subjectivism and the former's commitment to a critical foundation of the human sciences. Weber is given too much credit for resolving the problem of historicism with his notion of *Wertbezogenheit*, whereby values and beliefs are supposed to be treated as "facts" among other facts. The author berates Meinecke for narrowing and sanitizing historicism, but she herself neglects the wider European orbit of José Ortega y Gasset, Benedetto Croce, Raymond Aron, R. G. Collingwood, and Karl Löwith. Although not necessarily within the German historicist tradition, these figures did make its full dimensions once again palpable.

Wittkau suggests that, while historicism waxes and wanes periodically, its vistas and problems are now integral to our culture and way of looking at ourselves. If modern Western thought and culture continue to remain infused with historicist assumptions (perhaps tenaciously lodged within the "posthis-

toire"), the author has performed a signal scholarly service in so lucidly rendering their overall alignments and implications.

MICHAEL ERMARTH
Dartmouth College

GARY E. WEIR. *Building the Kaiser's Navy: The Imperial Navy Office and German Industry in the von Tirpitz Era, 1890–1919*. Annapolis, Md.: Naval Institute Press. 1992. Pp. xii, 289. \$36.95.

This is the first comprehensive discussion of the relationship between the German navy and industry, or the naval-industrial complex, in the period from 1890 to 1919. Gary E. Weir has made extensive use of the official naval documents and the private papers of key naval figures in the Militärarchiv-Bundesarchiv in Freiburg im Breisgau and the Krupp archives in Essen. The ground is covered in eight chronological chapters, each preceded by a brief discussion of the political background and followed by sections discussing topics such as the protocol system through which the Imperial Naval Office sought to control quality, prices, weights, and delivery of the products of the industries involved; attempts to fight monopoly, particularly of the armor production; research and development; labor; and raw materials. This organization inevitably involves repetition and some fragmentation, but these disadvantages are offset by a strong conclusion that brings together the themes for the entire period. A thematic discussion of these subjects would not have allowed their consideration in the light of changing political and military circumstances. Very useful statistics and tables follow in the appendixes.

Alfred von Tirpitz is rightly charged with imposing an unworkable strategy on the imperial navy through his concentration on battleships and refusal seriously to consider alternatives, particularly submarines, and his neglect of technical innovation. He is further charged with leaving most of the research and development with private firms, with the glaring result that German industry failed to produce an effective counterpart to the British Parsons turbine. But he gets high marks for his political and administrative ability and for acquiring a first-rate navy within the limits he had stipulated. Through the protocol system and the employment of imperial shipyards he generally managed to retain the upper hand in dealing with the private industrial sector; only the Krupp-Dillinger armor-producing combination proved a tough challenge, but by resorting to American competition, making use of Reichstag pressure, and by raising the possibility of involving the Thyssen firm, he induced it to a substantial reduction of prices.

The navy managed to maintain the quality control of its ships during the war in spite of increased difficulties. Unlike other historians, Weir considers the Scheer plan of September 1918—which called for

the building of 333 submarines in 1919—as serious, but myopic, as the imperial leadership was unaware that the war was lost.

Weir could have provided more adequate proof that industrial pressure contributed to Tirpitz's adherence to the battleship priority. Surely submarine construction would also have contributed to industrial profits. A few errors should have been expurgated by the author or by readers of the manuscript: the Supreme Command was not replaced by the Admiralty Staff in 1899 (p. 37) but rather by it and six different agencies, as is shown by the chart on page 211. Most of Germany's best capital ships did not join the fleet between 1906 and 1912 (p. 81) but between 1908 and 1914. Holland was not occupied by the Germans during World War I (pp. 151, 165–66). The Scheer plan had nothing to do with bringing "the Triple Entente to the bargaining table" (p. 196), since Soviet Russia had concluded peace with imperial Germany in March.

IVO N. LAMBI
University of Saskatchewan

WILLIBALD GUTSCHE. *Ein Kaiser im Exil: Der letzte deutsche Kaiser Wilhelm II in Holland: Eine kritische Biographie*. Marburg: Hitzeroth. 1991. Pp. 277. DM 38.

Historian-biographers are confronted with the choice of judging their subjects, seeking to understand them, or attempting some mixture of judgment and understanding. As the subtitle of his book announces, Willibald Gutsche sits squarely on the bench of historical judgment. Making little or no effort to understand Wilhelm II in exile, Gutsche's book is a sustained critique of the ex-Kaiser, his family and associates, and monarchist groups in Germany, who all to varying degrees collaborated with the Nazis in an effort to restore the Hohenzollern dynasty.

Gutsche writes with glowing, if subdued, indignation. The ex-Kaiser's life in Holland is described, from his conspicuous consumption to his daily tree cutting (which helped Wilhelm overcome boredom and depression). His racist, anti-Semitic diatribes are recounted (which served as explanations for Germany's defeat and Wilhelm's fate). His political machinations are detailed (as Wilhelm sought to return to the position he had been born and raised to fill). The ex-Kaiser who emerges from these pages is a pathetic figure. His hope of being restored to the throne was illusory from the beginning, and, with history having passed him by, Wilhelm became a pawn in a game he did not understand.

Despite the fact that Wilhelm II appears something of a historical irrelevancy here, the author seeks to demonstrate the Kaiser's significance in two ways. Gutsche contends that the efforts of Wilhelm and his supporters to restore the monarchy by collaborating with the Nazis undermined the Weimar Republic and rendered the Nazis "hoffähig." Gutsche also implies

that the history of the Kaiser in exile confirms that important continuities in ideology and ambition connect the second and the third German Reichs.

Although there is something to both of these claims, Gutsche overstates the case. Clearly Wilhelm yearned to return to power and to see the Weimar Republic replaced by a monarchy. And clearly he sought to use the Nazis to achieve these ends. But the author tends to play up Wilhelm's fitful collaboration with Hitler and to play down his contempt for the former corporal as well as his denunciations of the more radical and brutal features of Nazism. Moreover, in seeking to establish connections between Nazi and Wilhelmine Germany, one cannot simply assume that the views expressed by Wilhelm and his associates after 1918 were identical to those they held before the collapse of the *Kaiserreich*. Finally, it should be noted that criticizing the last German Kaiser is considerably easier than understanding him.

Despite these reservations, I found Gutsche's book entertaining and useful. Its long-term significance may be more historiographical than historical, however, for it was written by an East German scholar under the immediate impact of the collapse of the German Democratic Republic. It is instructive to compare this work with Gutsche's full-scale biography of the Kaiser written before the "*Wende*," *Wilhelm II: Der letzte Kaiser des Deutschen Reiches; Eine Biographie* (1991). The present book will thus interest historians of Germany both as an engaging critique of Wilhelm II in exile and as a revealing document on the effect of the end of "actually existing Socialism" on East German historical writing.

THOMAS A. KOHUT
Williams College

ERNST NOLTE. *Martin Heidegger: Politik und Geschichte im Leben und Denken*. Berlin: Propyläen. 1992. Pp. 330. DM 48.

The first thing one can say about Ernst Nolte's contribution to the burgeoning controversy over Martin Heidegger's commitment to National Socialism is that he has chosen an interpretive tack that few respectable scholars would dare. Heretofore, Heidegger's defenders have tried to minimize the significance of his Nazism via two primary strategies. For years they claimed that Heidegger's commitment to German fascism was insincere. After the recent exposés by Victor Farias (*Heidegger and National Socialism* [1987]) and Hugo Ott (*Martin Heidegger: Unterwegs zu seiner Biographie* [1988]) made such rationalizations untenable, the next line of defense was to concede Heidegger's personal failings, while claiming that the philosophy itself remained wholly unimpugned; as if such a paramount life-choice could have remained a matter of philosophical indifference to the man primarily responsible for twentieth-century *Existenzphilosophie*. Hence, of late, this strategy too has

been coming apart at the seams (see my own *The Politics of Being: The Political Philosophy of Martin Heidegger* [1990] and Tom Rockmore, *On Heidegger's Nazism and Philosophy* [1992]). Nolte's truly original and shocking gambit in this debate is to concede both points—that not only was Heidegger a National Socialist but also a "principled" one, that is, his Nazism was born out of philosophical conviction—and then to claim that, given the historical situation (which Nolte essentially defines in terms of liberalism's collapse and the threat of world communism), Heidegger was "right" to have made such a political choice.

Let us not forget with whom we are dealing here. Nolte, whose first book was *Der Faschismus in seiner Epoche* (1963; translated as *Three Faces of Fascism*), went on to write *Germany and the Cold War* (1974), which Peter Gay described as "a massive and sophisticated apology for modern Germany" insofar as it sought to "humanize" Nazism's misdeeds by comparing them with the crimes of others. Of course, it was the same revisionist agenda that lay behind his infamous essay, "Die Vergangenheit die nicht vergehen wird" ("The Past that Refuses to Go Away"), that provoked the German *Historikerstreit* of 1986–87. There, as is by now well known, Nolte sought to relativize the "so-called annihilation of the Jews" (see the English version of the essay, "Between Myth and Revisionism," in H. W. Koch, *Aspects of the Third Reich* [1985]), by claiming that the communist gulag was more "original" than Auschwitz; hence, that "the 'class murder' of the Bolsheviks [was] the logical and factual prius of the 'race murder' of the National Socialists." In the same essay, Nolte reintroduced the revisionist canard (a subterfuge favored by R. Faurisson and others) that Hitler's handling of the Jewish question (their detention and relocation in concentration camps) was "justified" as a result of (Jewish Agency president) Chaim Weizmann's recommendation in 1939 that in the event of a war the Jews should support the Western democracies. He even speculates that Hitler's response to this Judeo-Bolshevik threat from the east—the Holocaust—was, while irrational and excessive, comprehensible in terms of Hitler's own fear of becoming a victim of an "Asiatic deed" (that is, the Bolshevik "class war").

Nolte's revisionist claims therefore have constantly tried to relativize the significance of Nazi atrocities for both German and world history. Lurking behind such efforts has always been the insinuation that, when seen in a comparative light, German errors were never so egregious as the non-German world makes them out to be. Yet in this book about Heidegger—which really contains nothing new about Heidegger's case per se—Nolte ups the ante considerably. By contending that Heidegger's political option for Nazism in 1933—and, by extension, Germany's—was a politically sensible one, he merely uses Heidegger's case as a pretext for an elaborate and cynical exercise in German neonational exoneration.

The version of National Socialism that Nolte em-

braces is one that is nonracist, and thus freed of the "excesses" of the Judeocide. He claims then that this is precisely the variant of National Socialism that Heidegger briefly espoused. The book's truly outlandish claim, however, concerns a qualified justification of Nazism as something that was fundamentally provoked by communism and which, in the last analysis, turned out to be the lesser historical evil. In order to make this argument hold, Nolte engages in some remarkable distortions of logic and historical fact. He defines communism as the "grand solution" ("*grosse Lösung*") and Nazism as the "small solution" ("*kleine Lösung*"), and he argues that the latter was historically legitimate as a response to the former: "In comparison [with communism] the German revolution of National Socialism was modest, even meager in its . . . goals—the restoration of Germany's honor and equality of right—and moderate in its methods" (p. 123). As the "small [or less radical] solution" to the problems of liberalism's decay, Nazism was the lesser evil, argues Nolte. Clearly, to identify the primary ideological thrust of National Socialism as anticommunist rather than anti-Semitic is demonstrably untenable. But it furthers Nolte's revisionist agenda (one with which his readers are only too familiar by now), which is embodied in the claim that Nazism was a historically defensible phenomenon in light of its unyielding anticommunist ideology. Here, Nolte obviously feels that, in view of communism's recent collapse, the time is ripe for a major rehabilitation of the National Socialist legacy. For Germany (as well as for other European nations), fascism was merely a case of "legitimate defense" in light of Bolshevism's open declaration of "class war." It was, as it were, the only reliable means that the non-Asiatic bourgeoisie had at its disposal to counteract the threat of Asiatic "Red Terror," albeit a means that, in its zeal, took on some of the characteristics of the phenomenon it set out to oppose. In the final analysis then, Heidegger was correct to throw in his lot with National Socialism as the last hope for Western civilization against the rising tide of communism, for communism, as opposed to Nazism (the "*kleine Lösung*"), really would have meant "the end." Because of their manifest weakness and irresolution, the parliamentary democracies of the West could never be entrusted with this world-historical task (consider Nolte's description of "the liberal system," which he characterizes in a manner worthy of Oswald Spengler, Arthur Moeller van den Bruck, or Carl Schmitt, as a "problematic-problematical societal order" [p. 148]).

Along the way, Nolte makes some truly astounding claims. Socialism is "fundamentally a theory of annihilation" (p. 149). It is unfair, he contends elsewhere, to expect Christians not to be anti-Semitic given the doctrinal fact that Christianity conceives itself as existing historically in a state of tension with Judaism (p. 30).

Nolte's arguments and positions should not be taken lightly, for they eerily illustrate the virulently

rightward ideological shift that has taken place in Germany since the 1980s (Bitburg, the *Historikerstreit*) and in the aftermath of reunification. Above all, what Nolte's books teach us is that revisionist lore, which for decades has been confined to journals of the extreme Right, now occupies center stage in the German public sphere.

Finally, Nolte is thoroughly familiar with the illiberalism of the "German ideology" (from Fichte's *Addresses to the German Nation* to Carl Schmitt's *The Concept of the Political*) in a way the philosopher's many apologists and defenders are not. It is in this respect especially that his well-informed arguments concerning Heidegger's ideologically grounded partisanship for Germany's "National Revolution" deserve to be appreciated; this despite Nolte's own extremely troubling justification of that partisanship.

RICHARD WOLIN
Rice University

WALTER OTTO WEYRAUCH. *Gestapo V-Leute: Tatsachen und Theorie des Geheimdienstes; Untersuchungen zur Geheimen Staatspolizei während der nationalsozialistischen Herrschaft*. Paperback edition. Frankfurt a.M.: Fischer Taschenbuch. 1992. Pp. xiv, 140. DM 14.80.

This study by Walter Otto Weyrauch represents a unique contribution to the growing literature on Gestapo informants. In contrast to two other works on the activities of the Gestapo in Frankfurt am Main (Thomas Klein, ed., *Die Lageberichte der Geheimen Staatspolizei über die Provinz Hessen-Nassau 1933–1936* [1986], and Adolf Diamant, *Gestapo Frankfurt a.M.* [1988]), Weyrauch's book is based on a still-classified Frankfurt Gestapo card file on 130,000 individuals. This collection, an index to larger missing case files created by the Gestapo responsible for the governmental district of Wiesbaden, included 1,200 cards on official informants. The author, a German law clerk in Frankfurt in 1945, analyzed these cards for the American occupation authorities and in 1986 published a summary of his findings in an article in the *Columbia Journal of Transnational Law*.

Weyrauch concludes that paid Gestapo informers (V-persons) were recruited from foreigners living in Germany, political opponents of the regime, and racially and religiously persecuted groups. Swiss citizens were prominent among informers from neutral nations. Among enemy alien collaborators, the largest percentage came from Eastern European countries, but many French and even some Americans were listed as informers. Like the Stasi of the former German Democratic Republic, the Gestapo was able to recruit a sizable number of collaborators from the political opposition. Most of those recruited from racial and religious groups anathema to the Nazi regime were Jews, although some Catholic priests worked for the Gestapo. Weyrauch claims that three categories that were of little practical use to the

Gestapo—convinced Nazis, Jehovah's Witnesses, and members of Sinti (gypsies)—were not listed as informers. The author never gives precise percentages for his various categories, and he uses only well-known national examples of Swiss and Jewish collaborators as illustrations. Weyrauch concludes that most informants cooperated with the Gestapo because of coercion rather than for financial benefits.

Several of the author's findings are confirmed by other studies. *Verfolgung und Widerstand unter dem Nationalsozialismus in Baden* (1976), a collection of Karlsruhe Gestapo situation reports edited by Jörg Schadt, included references to Swiss and French V-persons. The role of Jewish informants was revealed as early as 1950 in postwar trials of Frankfurt Gestapo officials and examined again by Peter Wyden's *Stella* (1992). Unfortunately, Weyrauch ignores much of the recent literature on the Gestapo, some of which contradicts his findings.

Weyrauch did not use Klein's work, which specifically mentioned the use of SS men as Frankfurt Gestapo informers. Weyrauch's thesis that official informers were most responsible for undermining public trust is not confirmed by either Reinhard Mann (*Protest und Kontrolle im Dritten Reich* [1987]) or Robert Gellately (*The Gestapo and German Society* [1990]). Mann's study, which Weyrauch did not use, showed that only 15 percent of all Gestapo cases were initiated by V-persons or Gestapo observations. Most Gestapo investigations were initiated by volunteer informers, a category excluded from Weyrauch's analysis. Weyrauch cites Gellately's work in one footnote to document the scarcity of Gestapo case files, but he ignores Gellately's examples of informants who collaborated for ideological or personal reasons. It is unfortunate that Weyrauch failed to integrate his interesting data with the scholarly literature on this topic.

JOHN PETER HORST GRILL
Mississippi State University

DAVID BANKIER. *The Germans and the Final Solution: Public Opinion under Nazism*. (Jewish Society and Culture.) Cambridge, Mass.: Blackwell. 1992. Pp. vii, 206. \$44.95.

In the last decade or so, historians have sought to deal with the persecution of the Jews in Nazi Germany by moving away from broad national accounts to explore specific classes and social groups, as well as various localities and regions. This change of perspective has been accompanied by a growing diversity of theoretical approaches, from quantitative analysis, to women's history, oral history, and history of everyday life. The point of departure of much of this work is a belief that little more is to be gained by re-running the kind of grand narratives published in the first decades after 1945. Although proponents of the newer approaches are by no means unanimous in

their views, it is fair to say that they seek to get behind abstractions about "the German people," "the SS-state," and even the role of prominent leaders, to study the Third Reich "from the bottom up." Paying more attention to social context—a stand-by axiom of even the most traditional historian—produces complex, demystified, and convincing accounts.

David Bankier, however, returns to the method of grand synthesis and rejects in-depth social analysis. He links together snippets of information from all over Germany, but it is hard to develop a compelling story with this level of decontextualization, and the result is a loss of concreteness and an uncomfortable number of generalizations. Readers are presented with too many undefended assertions, the most troublesome of which are at the end of the book. There we are informed that "large sectors of German society were predisposed to be anti-Semitic" (p. 155), indeed, that "most Germans were 'traditionally' anti-Semitic" (p. 156). This comes as a surprise because until this part of the book the impression Bankier conveys is that the effort to convert the German people to Nazi anti-Semitism was an uphill battle that generally failed. In fact, as the book proceeds, the broad social consensus often (mistakenly) thought to be the necessary basis for the persecution of the Jews is undermined, and one begins to wonder how it was possible for the regime to accomplish anything. By the end of the book Bankier is faced with a "paradox of how a population that was basically anti-Semitic rejected anti-Jewish propaganda" (p. 145). Perhaps that is why he sought a way out of the "paradox" by introducing Germans' putative "'traditional' anti-Semitism." He then resorts (as elsewhere in the book) to popular psychology and surmises that Germans refused to "succumb" because paying "attention to the anti-Semitic propaganda [would have] entailed [admitting to] an unpleasant awareness of the atrocities committed in the name of solving the Jewish question" (p. 146). We are also informed that the Germans' "deliberate escape into privacy and ignorance" and "lack of committed opposition to the persecution of the Jews largely *explains* [emphasis added] why so many deliberately sought refuge from the consciousness of genocide and tried to remain as ignorant as possible because it salved their conscience" (p. 156). There is no way to assess these kinds of statements because they cannot be supported by documentary evidence, nor are they the interpretive results of a psychoanalytically based study.

The book brings together most of the sources that survive on "public opinion" and the persecution of the Jews in Nazi Germany, and the picture, already well known in outline, remains essentially unchanged. Nevertheless, in assembling these sources Bankier has performed a useful service to the continuing discussion as to the nature and extent of ordinary Germans' knowledge of, complicity in, and responsibility for the persecution of the Jews in Germany, and the subse-

quent escalation into the events that culminated in the "final solution."

ROBERT GELLATELY
Huron College,
University of Western Ontario

EDWARD N. PETERSON. *The Many Faces of Defeat: The German People's Experience in 1945*. (American University Studies, ninth series; History, number 88.) New York: Peter Lang. 1990. Pp. 369. \$72.95.

This is Edward N. Peterson's second book on the interaction between the victors and the vanquished in Germany following World War II. His first study focused on the U.S. occupation; it detailed the confusion of U.S. policy, the hardships (intended and unintended) that it imposed, and the need for the Americans to revise their policy in view of hard realities. This second book is broader geographically and somewhat narrower chronologically. It deals with the transition from war to peace and the early stages of military occupation in Germany. It is divided into six main parts. Five trace the policies of the powers directly involved in the postwar restructuring and administration of Germany—the United States, Great Britain, the Soviet Union, France, and Poland; the remaining part deals with the special situation of Berlin. The sections covering individual occupation powers are divided into chapters dealing respectively with the country's policy (official and public) vis-à-vis defeated Germany, its treatment of German POWs, and its policy toward the German population in its zone of occupation. The main emphasis is on the latter; throughout the book the author relies on an anecdotal approach, since, he feels, the stories "have a value beyond the enjoyment. Hundreds of small candles of common man/woman experience light up the otherwise dark corners of the people's past" (p. 5). Peterson effectively chronicles the cruelty inflicted on the defeated by the victors. From the German—and his own—perspective the British behaved most decently; the Americans were alternately harsh and generous; the French were vindictive and nasty; the Russians, followed closely by the Poles, were most vicious and brutal.

Peterson's larger goal is to show how war harms all who are involved: not only do the defeated experience undesirable consequences but so do the victors as well, in the form of the abuse of power and corruption of their ideals. It is useful to be reminded of this, to be shown how the Germans suffered and that it was not only in the eastern zones that the victors were cruel. Yet Peterson's account is unsatisfactory in that it remains—in spite of disclaimers—too close to, and too colored by, the sources he uses. Anecdotal evidence necessarily has a narrow focus and, like most *Alltagsgeschichte*, this book provides too narrow a perspective. Intelligence reports and other accounts from the occupation period repeatedly ex-

pressed astonishment at the apparently limitless self-pity of the Germans and their refusal to take responsibility for the barbarity of the Third Reich. German accounts of the period self-servingly stressed how Germans were suffering, artfully avoiding the question of why, an approach that is currently enjoying a certain revival among some German historians and politicians. According to the tortured logic of postwar German victimology, the Germans were victims too and therefore not accountable for the suffering of their victims. Two wrongs, of course, do not make a right. But it must not be forgotten that World War II was started by the Germans, not the postwar occupation powers, and that the cause of German misfortune after 1945 was not, as many Germans seemed to imagine, an inexplicable act of fate, but the direct consequence of their own actions before 1945.

JAMES M. DIEHL
Indiana University,
Bloomington

CHRISTINE SMITH. *Architecture in the Culture of Early Humanism: Ethics, Aesthetics, and Eloquence 1400–1470*. New York: Oxford University Press. 1992. Pp. vii, 298. \$35.00.

Christine Smith introduces her enlightening analysis of the role of architecture in the ethical and cultural considerations of humanists of the first three quarters of the fifteenth century with a description of the Gothic cathedral of Florence by the Italian architect and humanist Leon Battista Alberti in his dialogue of 1441 or 1442 on the tranquility of the soul. The cathedral becomes an allegory for the tranquility of the soul. That Alberti, renowned as one of the architects to revive the classical style of architecture, should have chosen perhaps the most Gothic building in Florence for praise with words such as "grace" and "majesty" seems surprising. He is noted, however, for viewing history as a continuum and failing "to recognize the existence of any separate period between Antiquity and the present" (p. 69), so his architectural criticism is not dependent on historical style, but instead on aesthetic qualities.

Earlier Alberti, in the preface to his treatise on painting from 1436, had praised Filippo Brunelleschi's dome of the cathedral as evidence of the outstanding cultural accomplishments of Florence at that time. That architecture, traditionally considered to be one of the base mechanical arts, should have been singled out to represent the cultural achievement of the city demonstrates the impact of Greek thought on the early Renaissance, as does the increased interest in mechanics as a university discipline. This was the period of the rediscovery of the Greek treatises on mechanics, such as the pseudo-Aristotelian *Mechanical Problems* and the works of Archimedes. In fact, the *Mathematical Collection* of Pappus probably was of

assistance to Brunelleschi during the construction of the dome.

Smith challenges the traditional dogma that Alberti's thought was determined by Platonism to the exclusion of Aristotle. She asserts that Alberti was eclectic like other early humanists who considered Aristotle, Cicero, and St. Augustine, rather than Plato, as their major masters. Smith's superb chapter on the rebuilding of the Tuscan town of Pienza for Pope Pius II by architects familiar with Alberti's ideas illustrates the prevalence of the aesthetic principle of *varietas* that was a maxim of rhetoric as enunciated by Cicero and Aristotle. The buildings vary in architectural styles determined by their different functions or social status. Smith reviews the traditional evaluation of the urban plan of the renewed Pienza as an ideal Neoplatonic design with a fixed, static viewpoint, and she convincingly shows that this does not correspond to one's visual approach to the central piazza, but instead describes the design as "optical rather than mathematical, dynamic rather than static, and unfolds in a temporal sequence determined by the spectator's physical movement" (pp. 116–17). Such a phenomenological approach has little to do with Neoplatonic idealism.

The last third of the book focuses on the Byzantine contribution to the Italian Renaissance cultural tradition, admitting that Latin authors were always the principal inspiration. The great Byzantine scholar Manuel Chrysoloras imbued young Italian scholars, such as Leonardo Bruni, Guarino Veronese, and Ambrogio Traversari, with the art of eloquence, which was the foundation for their literary and moral achievements in the early fifteenth century. It was the circle of mainly young humanists around Chrysoloras who wrote about the environment to demonstrate their civic pride. In 1411, Chrysoloras wrote his own *Comparison of Old and New Rome* to better the relations between his native Constantinople and Rome. This required comparing the two cities at the same time each was being praised. An English translation is furnished in an appendix to permit the reader to follow how skillfully Chrysoloras walked his "tight-rope."

As Smith carefully indicates, this study is not a traditional history of early Renaissance architecture, but rather an illuminating and discerning consideration of what contemporaries believed was important in the built environment, and what architectural imagery could contribute to their moral and political ideals. The modern historian's concern for historical style is demonstrated to be lacking during this period of the early Renaissance.

DAVID R. COFFIN
Princeton University

THOMAS KUEHN. *Law, Family, and Women: Toward a Legal Anthropology of Renaissance Italy*. Chicago: University of Chicago Press. 1991. Pp. xiii, 415. \$40.00.

Thomas Kuehn's collection of articles on law, family, and women weds the disciplines of social history, legal history, and legal anthropology in order to demonstrate that the law is an active, vital entity in society. Kuehn's focus on the important social role of the law is offered as a corrective to the deficiencies of the *Annalists*, for whom the law is a superfluous superstructural feature; some social historians who are largely ignorant of the part played by the law in the situations they examine; and legal historians who take a narrow, normative approach to the law. Legal anthropology, with its conception of legal systems as either transactional processes or functional structures, is also rebuked for losing sight of the significance of legal norms in complex societies, such as that of Renaissance Florence.

The promise of this interdisciplinary marriage is achieved, albeit unevenly, through Kuehn's intelligent use of a variety of demanding documentation: the writings of jurists, communal statutes (legislation), notarial documents, family journals, letters, and *consilia* (commissioned opinions of jurists whose job it was to find the law relevant to particular complex disputes). Social historians are likely to find most interesting and accessible the sections of the book that treat the law and the family. Kuehn's success is most limited—and he acknowledges this—in the essays dealing with women, where, after walking a fine line between exploiting the materials of legal history for the purposes of social history and exploring the problems of understanding the law as a system of norms, he indulges the latter interest.

Kuehn shows the courage of a big-game hunter in setting off to follow law, a rationalized system of norms, into actual cases, thickets where the rampaging beasts of private interests—honor, revenge, and avarice—snarlingly confront the principles of impartial justice and equity, seeking to recast them in self-serving terms. Few legal historians have been willing to attempt such a dangerous expedition. Perhaps the daunting image of the serpent devouring its own tail—how overreliance on construing human actions through the optic of rationality ends in revealing the predominance of irrationality—has unduly haunted the imaginations of other scholars. And the law itself, in terms of ideology and substance, is but a set of rationalized and institutionalized interests designed to maintain the gender, age, and class distinctions of a society. Kuehn emphasizes the resultant ambiguity, which emerges as an important element in historical interpretations; he does not try to minimize its impact as many historians do.

Nonetheless, the major theme running through these essays is that significant aspects of clashing interests, useful to social historians, are revealed during instances of conflict, along with ambiguities. Conflict is a normal component of human relations, Kuehn argues; legal systems attempt to resolve these conflicts according to norms. But, in Quattrocento Florence, this process was buttressed by the operation

of an informal system of arbitration aimed at resolving conflicts through compromise, saving "multiplex" (p. 20) relationships that were too easily sacrificed in the resentment attending litigation. The two systems were elided in fifteenth-century Florence; in dispute resolution more was involved than the law, more was at stake than justice. For example, concern for family honor and competition for the exercise of familial power emerge as prime motivators in Remigio Lanfredini's decision to castigate his father and break off all relations with his paternal household. The Lanfredini family was dysfunctional because the father was timid in the exercise of his authority. Likewise, from the point of view of a bastard, such as Leon Battista Alberti, a family could be dysfunctional when avarice drove some members to despoil others of their rightful inheritance (the determination of which was often problematic, the subject of litigation). Alberti's famous image of the ideal family (*Della Famiglia* [1436]), Kuehn perceptively argues, was also subtly designed as a condemnation of his treatment at the hands of his uncles.

Thus, it is not useful, according to Kuehn, to attempt to invent a stereotypical Renaissance family; Florentine families were different—some successful, others not—with different resources, problems, and strategies (p. 136). In the section on the family, and in his last essay on women, Kuehn discloses that family identity itself was challenged for motives of revenge or profit, especially when the control of wealth (property or money) devolved through inheritance to legitimized bastards or women.

But I can only begin to display the treasure contained in Kuehn's essays. Untouched above is material that explores the concept of property in Quattrocento Florence, the extent of violence among patrician families, the continued existence of the *patria potestas* over married women, and the guardianship role for women of the male *mundualdus*. The University of Chicago Press has performed a service in compiling this volume.

JOHN K. BRACKETT
University of Cincinnati

ANN E. MOYER. *Musica Scientia: Musical Scholarship in the Italian Renaissance*. Ithaca: Cornell University Press. 1992. Pp. 325. \$44.50.

Ann E. Moyer's rich, detailed, and almost complete survey of the extensive, diverse, and largely neglected literature on music from classical antiquity to the end of the sixteenth century is well written, organized, and documented. It is based on a complete command of the primary sources, both manuscript and printed, and of the extensive secondary literature, both early and recent. It is a major contribution to ancient, medieval, and Renaissance scholarship and an indispensable tool for any student or scholar in need of precise and reliable information about its complex

and important subject. The volume contains at the end a glossary, a select bibliography of manuscript and printed sources and of secondary works, and an index. The many links that connect musical theory with the other branches of the Quadrivium (arithmetic, geometry, and astronomy), with natural philosophy and early modern science, with the humanistic, philological, and other cultural currents of the time, and with the performers and composers of music, are clearly described and disentangled.

I hope the author will publish in the future a critical edition of the treatise of Raffaele Brandolini that she analyzes in detail on the basis of a unique manuscript.

I should like to mention a few minor details that might be added in a future revised edition of this comprehensive study. In the bibliography, I missed the important works on medieval and Renaissance music by Gustave Reese. There are also a few unpublished authors that might be added to Moyer's list, such as William of Lucca, Johannes Ciconia, Johannes Bonadies or Godendach, and Johannes Baptista Burana. Moreover, the link between Johannes Galliscus and his teacher Vittorino da Feltre might be worth mentioning. As to Marsilio Ficino, he might have deserved a fuller treatment. His *Platonic Theology*, his commentaries on Plato, and his *Letters* had a wide and strong influence throughout the sixteenth century and even later, and his statements that music was one of the arts revived in the fifteenth century, that it was linked with medicine and theology, and that musical beauty had the same rank as visual or intellectual beauty, were by no means forgotten by his readers. Finally, in a special treatise on music preserved in only one manuscript, Ficino follows the taste of his time by treating the third as consonant and the fourth as dissonant.

Moyer has made an excellent contribution that will serve as an indispensable reference work for a long time to come.

PAUL OSKAR KRISTELLER
Columbia University

JACK M. GREENSTEIN. *Mantegna and Painting as Historical Narrative*. Chicago: University of Chicago Press. 1992. Pp. xiv, 301. \$35.00.

In this exhaustive and erudite iconographical study of Andrea Mantegna's *Circumcision of Christ* (ca. 1470), Jack M. Greenstein argues that the painting is one of the earliest fulfillments of Leon Battista Alberti's concept of *historia*: a narrative true to its literal sense as something "done and seen," yet, by means of the artist's formal invention, simultaneously true to its spiritual sense (pp. 186–89).

On the literal level, according to Greenstein, the painting represents Simeon taking a scalpel in his right hand to perform the circumcision while assisted by a Levite holding a tray with scissors and gauze;

simultaneously (and here the reading is problematic) Simeon grasps with his veiled left hand "the child's thigh as if to take him into his arms—an action that initiates the rite of presentation" (p. 107), a rite to be completed by the sacrifice of two pigeons held in a basket by Joseph standing to the extreme left. By recalling the procession of Candlemas, the three women on the right, including the Virgin and the old prophetess Anna, are seen as representing the purification of the Virgin. Because she has no textual basis, the third woman is explained as an Albertian narrator, who by the action of solicitude for her young son also represents Jewish moral law. The setting, Greenstein claims (unconvincingly), represents a reconstruction of the temple of Jerusalem based on Josephus, probably even specifically the court of women, with the ark of the Law behind closed doors. The simulated reliefs of the sacrifice of Isaac and the promulgation of the Law by Moses signify respectively the institution of temple sacrifices and the obedience to Moses's law by the tribes of Israel. In general, the left half of the painting represents the Israelites' relationship to God, the right half their relationship to each other.

Greenstein then reads each narrative element on a spiritual level. The circumcision prefigures the eucharist; the sacrifice of Isaac, Christ's sacrifice; the pitcher on the altar, baptism; the paten-like tray of the Levite, the mass; the Levite, the priesthood; the white Levite's robe, the robes worn by neophytes after baptism and by acolytes and priests at mass as a sign of purity; the young woman and son, *amor proximi*; the Virgin, *amor dei*; the number eight (eight figures, and the circumcision eight days after the nativity), resurrection. The architectural setting is said to be an inversion of a triumphal arch to show the humility of Christ's volitional submission to the circumcision, and the partially eaten bread ring of the small boy is a "symbolic inversion of the Host" (p. 219) showing the Old Law's inability to satisfy spiritual hunger. In general, the left half of the painting prefigures the new dispensation, the right Christian community.

From a semiotic viewpoint, such "symbolic inversions" and rigid bipolar interpretations of every detail are highly questionable. Furthermore, if the sacrificial pigeons refer to the purification (and not the presentation), and if the gesture of Simeon's veiled left hand relates only to the circumcision—and Christ's action of withdrawal from Simeon supports this view—then the composition conflates only the circumcision and the purification, for which there is a visual tradition (as in Greenstein's figures 44 and 45). This would invalidate Greenstein's major claim that the painting is the first conflation ever of the circumcision, presentation, and purification, and would make unnecessary his unlikely proposition that Mantegna was an original theologian and biblical scholar intimately versed in Jewish law and custom, Christian theology and liturgy, and the writings of Josephus, Origen, and perhaps even pseudo-Matthew.

Since Greenstein's novel (and convincing) reading of Alberti's *historia* depends on a detailed understanding of classical and medieval theories of history and biblical hermeneutics, he must also posit that Mantegna had "an intellectual outlook" equivalent to "a humanistically educated audience"; that Mantegna was "one of the most important historical thinkers of his time"; and that he was "one of the strongest Renaissance readers of Albertian tradition" (pp. 8, 13). So fully did Mantegna accept the "representational imperatives" of Alberti, Greenstein argues (unpersuasively), the artist used two perspective viewpoints: one low for the figures, making them "distinct presences" to emphasize the literal sense, another high for the setting, making it "less immediately present" to emphasize the spiritual sense (p. 209).

The obsessive concern for Mantegna's individuality, originality, and artistic genius—phrases such as "for the first time in the history of art" (for example, pp. 84, 97, 208, 220) ring like a leitmotif throughout the book—underlines Greenstein's masculinist interpretive ideology. Patriarchal discourse such as "Mantegna realized that" or "understood that" or "felt justified in" (pp. 181, 206) also shows that this ideology is both internalized and institutionalized, that Mantegna—the great thinker and theologian—is a stand-in for Greenstein the academic.

LOREN PARTRIDGE
University of California,
Berkeley

SAMUEL K. COHN, JR. *The Cult of Remembrance and the Black Death: Six Renaissance Cities in Central Italy*. Baltimore: Johns Hopkins University Press. 1992. Pp. xiii, 429. \$47.50.

This is an important book, although it raises numerous questions and many doubts. The reader might be well advised to begin by consulting table 5 (pp. 76–77). There Samuel K. Cohn, Jr., sets out his data, listing them chronologically. He analyzes wills from six central Italian cities: Arezzo, Assisi, Florence, Perugia, Pisa, and Siena. His generalizations and conclusions are based on sizable samples for certain decades and meager ones for others. The uneven character of his evidence is heightened by the fact that in certain archives (the Archivio di Stato in Florence) it is only possible to examine a few documents each day. If one is reading loose documents, such as the Diplomatico, the number is restricted to eight. This means that Cohn's analysis must perforce be based on less than ideal documentation. After perusing table 5 and chapter 10, which outlines the sources, the reader must then consider two other matters: first, what was the extent and character of the will-making population in these six cities of Tuscany and Umbria? Second, what role did the professional notary play in shaping and even transforming the last will and testament? These are questions to

which different readers will have varied answers, but the conclusions reached will influence any reading of the quantitative analysis confidently proffered by Cohn.

The author is to be commended for the breadth of his inquiry and the boldness of his interpretation. Naturally enough, his brave effort is quite successful at certain moments and less so at others. His forays into art history are in general unrewarding. Employing wills from Tuscany and Umbria, he would challenge conclusions drawn by art historians. But since he lumps together petty and grand legacies of bequests for religious art, there is no proper distinction between a major funereal monument and a modest funereal plaque. His critique of Millard Meiss is especially ill-advised, and his comments on Florentine art often miss the mark. Those who use computers must not abandon the realm of common sense. There is yet another vexing matter: Cohn argues for continuity insofar as bequests continue to be made to canonical and parish churches as well as to friaries. He challenges notions of an increase in civic charity on the ground that traditional bequests and legacies were made without interruption to those time-honored ecclesiastical institutions. Of course they were. After all, it was priests, monks, and friars who attended to the burial and commemoration of the dead. Donors and testators could only think of secondary concerns such as civic charity after they had tended to the primary concern of their own interment. Taking these facts into consideration, we observe a substantial increase in civic charity, especially in Florence, over the late fourteenth and early fifteenth centuries. What Cohn's statistics do confirm is the conclusion reached more impressionistically some time back that indiscriminate charity (gifts to Christ's poor) was in decline during the fourteenth century. Also substantiated is the declension of innumerable small bequests by a single testator to be distributed throughout the community. Support is also lent to the argument that there was an increase in legacies for dowering poor maidens.

Cohn locates the turning point in will-making during the second wave of plague striking Tuscany and Umbria in 1363, tracing the trajectory of this change from that date to 1425. Contending that the cult of remembrance and conception of afterlife reached full form over that half century or so, he draws parallels between these transformations and the direction taken by charitable legacies and bequests. Although he denies assuming a Burkhardtian stance, he does accent the ambition of donor and testator to gain fame and glory—secular immortality. The movement is one from more anonymous forms of giving to one in which the claims of ego are dramatically exercised. Great was the anxiety and huge the expense to forestall obliteration of one's earthly record. Testators exerted control over the living from beyond the grave leaving bequests to which many strings were tied. The living were bound to the dead and encour-

aged to work for the felicitous transit of the souls of the departed. Women were particularly targeted and offered incentives for continuing their devotion to the memory of a dead spouse. It should be noted, however, that the sample on which Cohn's conclusions are based can be very small. For example, Cohn finds only thirty-one testators in Florence between 1349 and 1363; his numbers for the earlier period are even more sparse. In Assisi there are only five for 1349 to 1362. Despite this I do believe that Cohn is correct in his assessment of general changes occurring from the mid-fourteenth century to the first part of the fifteenth century. Testators and donors did exercise extravagant ego claims, and it is surely legitimate to speak of the enhancement of a "cult of remembrance." As was mentioned, while acknowledging Burckhardt's conception of the quest for fame and glory, Cohn would offer a different explanation for this phenomenon. He argues that the answer is to be found in the testator's preoccupation with property and ancestry. One does not have to accept this explanation in order to benefit from the conclusions of this study. If one uses notarial cartularies, it is easy to come up with a property-oriented explanation, since that is what they tend to record.

The results of this investigation contain little that is novel, but they are useful. They serve as a challenge to recent scholarly efforts trivializing the study of Italian Renaissance history. General hypotheses have been downplayed and cultural developments explained through actions of small cliques. E. H. Gombrich would deny the legitimacy of the Renaissance as a period and describe it as little more than a movement. The results of Cohn's research add weight to an interpretation, now out of favor, focusing on the rise of individualism. This is all to the good and should elicit lively responses.

MARVIN B. BECKER
*University of Michigan,
Ann Arbor*

PETER MUSGRAVE. *Land and Economy in Baroque Italy: Valpolicella, 1630–1797*. London: Leicester University Press; distributed by St. Martin's, New York. 1992. Pp. viii, 202. \$54.00.

The Valpolicella is a valley in northern Italy well known today for producing one specialty: a light and amiable red wine. Peter Musgrave maintains that in the seventeenth and eighteenth centuries the people of the Valpolicella deliberately avoided just this kind of economic specialization. On the contrary, they wanted a mixed economy, a preference Musgrave claims was typical of premodern societies.

Why did the people of the Valpolicella, and other premodern peoples, want a mixed economy? According to Musgrave, economic historians (he does not name them) usually give the wrong answer to this question. They maintain (he says) that premodern

people were too dull to understand that if they would only specialize they could swiftly raise their living standards.

In fact, Musgrave argues, the people of the Valpolicella knew just what they were doing. They preferred a mixed economy because what they wanted above all was not wealth but stability and disaster insurance. They deliberately diversified their crops, farmed on more than one type of land (hills, plains, and so forth), sharecropped, and combined farming with other work, such as rearing silkworms, quarrying marble, and transporting goods on the Adige River. Musgrave claims: "This was no failure of initiative nor the result of blind conservatism, but the working out of a clear and rational economic and social strategy" (p. 135). And he believes that despite the conservative strategy, the Valpolicella economy was not merely stable, but prospered.

All of these points may be true, but Musgrave does not have the evidence to prove them. Apparently he was unlucky in the match of his topic and his archives, because he repeatedly comments that his sources are sparse and the story they tell is murky. His performance indicators are "partial" (p. 73). He is unable to quote landowners or writers of agricultural treatises who discuss a deliberate policy of risk-spreading.

Musgrave does not try to back up his contention that the Valpolicella system was typical "by extension of much of the rest of the Mediterranean" (p. 109). But then, he does not need to demonstrate it. That people in hand-to-mouth economies diversified their portfolios is hardly a startling discovery.

Musgrave's argument, then, is flawed. His description of the Valpolicella's mixed economy, however—the nuts and bolts, not the numbers and the generalizations—may be useful to other students of early modern European agriculture.

JAMES C. DAVIS
University of Pennsylvania

RONALD T. RIDLEY. *The Eagle and the Spade: Archaeology in Rome during the Napoleonic Era*. New York: Cambridge University Press. 1992. Pp. xxviii, 328. \$79.00.

The impact of Napoleonic reforms on the basic structures of what used to be the state of the church was the object of an exemplary book by the French historian Louis Madelin (*La Rome de Napoléon: La domination française à Rome de 1809 à 1814* [1906]). This enthralling volume had a kind of paralyzing effect, inasmuch as it appeared to be so exhaustive that further research on the same topic was deemed unnecessary. The fear to rehash the subject so brilliantly treated by Madelin, combined with the jingoism of Italian intellectuals reluctant to attribute to non-Italian factors the nineteenth-century rebirth of their nation, made the Napoleonic domination of Rome an unlikely research project. Only in 1969 was

the pioneering role of Napoleon in the Risorgimento fully acknowledged in a conference organized by the Accademia Nazionale dei Lincei (see "Problemi attuali di scienza e di cultura," *Atti del convegno: Napoleone e l'Italia* [1973], 179). The interconnection between the Napoleonic regime and the Risorgimento was stressed by Alberto M. Ghisalberti in his "L'era napoleonica e il Risorgimento italiano" (*Atti del convegno*, 13–24). This important essay inaugurated a new trend in historical studies, as is demonstrated by Fiorella Bartocchini's *Roma nell'Ottocento* (1985), which was dedicated to Ghisalberti. Another important contribution to the above-mentioned conference was Vittorio E. Giuntella's "Roma nell'età napoleonica," (*Atti del convegno*, 357–64). Yet in 1989, Giuntella could lament the fact that the Napoleonic age continued to be overlooked by Italian historians (see his "Presentazione," in C. Nardi, *Napoleone e Roma: La politica della consulta romana* [1989], vii).

Giuntella's remark sounded the more true when applied to the field of the history of archaeology. Although precious information on this topic could be gathered from Attilio La Padula's works on town planning (*Roma, 1809–1814: Contributo alla storia dell'urbanistica* [1958]; *Roma e la regione nell'età napoleonica: Contributo alla storia urbanistica della città e del territorio* [1969]), one can safely say that the full extent of Napoleonic archaeological activities in Rome was quite ignored, despite the fact that Marita Jonsson devoted to this topic one chapter of her volume (*La cura dei monumenti alle origini: Restauro e scavo di monumenti antichi a Roma 1800–1830* [1986], 41–96). Although Jonsson was able to condense a lot of information into a limited space, the subject remained open for a more in-depth treatment. This task has been accomplished by Ronald T. Ridley.

Ridley's beautifully printed volume is solidly grounded on archival and printed sources, duly listed in the "Select Bibliography" (pp. 313–20). Here one can find only one item missing, namely, Carla Nardi's book, which is important only for the political background of the facts related by Ridley. In his "Prologue," Ridley reviews previous studies and convincingly demonstrates that the terrain he explores is largely uncharted. Perhaps he is too severe with La Padula, who, in his books on city planning, gave precious information on archaeology, culled from the same archival sources that Ridley more fully explores.

Ridley illustrates in his "Introduction" the extent of Roman influence on every sector of French life, the events that led to the annexation of Rome to France in 1809, and the reforms promoted by the French government. He then stresses the basic disregard for Roman ruins during the papal regime. According to Ridley, a more positive attitude was adopted by the Papal State in the years 1800–09, under the pontificate of Pius VII, who had "an unprecedented concern for the major remains of classical Rome" (p. 35). This chapter serves as a kind of prelude to Ridley's story, which he begins to tell in earnest in the second

chapter, entitled "Commissions, Commissions, Commissions: The Administration of Antiquities under the French." This section contains interesting insights into the strengths and weaknesses of the Napoleonic bureaucracy. Among other things, Ridley demonstrates that the "incredible activity of the handful of leading administrators in Rome" could be jeopardized by the Parisian bureaucrats who had no idea of Rome and yet "claimed the right to control everything" (p. 47). According to Ridley, the remarkable accomplishments of the Napoleonic era were due not only to Frenchmen, such as the prefect of Rome, Camille Tournon, or Governor Sextius-Alexandre Miollis, but also to Italians, such as Carlo Fea and Giuseppe Valadier. The price exacted from the Roman workers was very high: "thousands of unemployed of all classes . . . toiled for little more than a franc a day, together with a midday bowl of soup" (p. 48).

Ridley examines the full range of the excavations and restorations made by the French government in his next chapter, entitled "Arches, Fora, Theatres, and Temples: The Monuments Cleared and Restored by the French." He provides individual assessments of the archaeological projects pursued during the Napoleonic regime: from the Arch of Dolabella to the Domus Aurea; from the Forum Romanum to the Theater of Marcellus. These projects entailed many quarrels among prominent antiquarians. Ridley discusses the most significant one in his fourth chapter, "The Great Colosseum Debate." This controversy, which concerned the level of the floor of the arena, involved Pietro Bianchi, Lorenzo Re, Carlo Fea, Luigi Martorelli, and Juan Masdeu.

In this thoughtful book, written in a clear and uncluttered style, Ridley provides a cornucopia of revealing items that help explain not only the history of archaeology but also the intellectual and socioeconomic life of early nineteenth-century Rome. The well-selected illustrations enhance the value of this volume, which can be recommended to specialists as well as a larger audience.

GUSTAVO COSTA
University of California,
Berkeley

SANDRO ROGARI. *Cultura e istruzione superiore a Firenze: Dall'Unità alla grande guerra*. Foreword by GIOVANNI SPADOLINI. (Politica e storia, saggi e testi, number 31.) Florence: Centro Editoriale Toscano. 1991. Pp. 284. L. 28,000.

A work that is part monograph and part collected essays, Sandro Rogari's book tells the story of an ancient and cultured city's conscious effort to become the intellectual hub of unified Italy. The story is told through biographical sketches of important intellectuals and scholars, from Gino Capponi at the time of unification to Pasquale Villari and Gaetano Salvemini

at the turn of the century. But there is also much information about the formal institutions around which Florentine culture and academic scholarship took shape during the second half of the nineteenth century.

The essays on institutional development are the most valuable part of the book. There is much new material to supplement and validate Eugenio Garin's and Giovanni Spadolini's earlier studies. And Rogari's discussion of the politics of higher education in postunification Florence offers opportunities for reflection on contemporary situations of developing countries organizing new systems of postsecondary education and developed countries trying to restructure older systems.

Tracing the growth of the *Istituto di studi superiori* and the *Scuola di scienze sociali*, out of which grew the University of Florence, Rogari develops two lines of inquiry. The first deals with the desire of Florentine intellectuals (not all of them native sons) to make Florence the undisputed center of a renewed Italian culture. Their enthusiasm was enhanced rather than dampened by the decision in 1871 to make Rome the permanent political capital. But it was not easy to gain the support of practical-minded Piedmontese politicians like Urbano Rattazzi, Quintino Sella, or Agostino Depretis, who generally favored investing scarce national resources in elementary and vocational secondary education. The political and curricular debates surrounding the creation of the two institutes whet the reader's appetite for further research not only in Florence but also in other capitals of pre-unification states.

Rogari's second line of inquiry deals with intellectual history of a more traditional kind. Through the study of leading scholars and scientists at the Florentine institutes, he explores philosophical currents and the relationships among theory, pedagogy, and politics. Especially interesting is the essay on the *Biblioteca filosofica*, an independent entity that enabled restless intellectuals like Giovanni Amendola to write and lecture in opposition to the state-funded, politically conformist academic establishment.

The weakest part of this volume is the essay on Salvemini and Italian intervention in World War I. Unlike the others, this essay does not present new insights or information and appears to have been added to the volume as an afterthought. But overall, this book demonstrates both the need and the feasibility of delving deeper into the history of modern Italy's educational and cultural institutions.

CLARA M. LOVETT
George Mason University

LOUISE A. TILLY. *Politics and Class in Milan, 1881–1901*. New York: Oxford University Press. 1992. Pp. xii, 355. \$49.95.

Louise A. Tilly has written a solid, well-researched study of the relationship between the development of the Milanese working class and the rise of the socialist movement in its difficult gestation period. Her book is also—and many readers will find refreshingly—traditional in its combination of social background with ultimately political findings. There is no breathtaking methodology, and while important sections draw data into clear analytical focus, theory is never allowed to overwhelm descriptive materials. Even women, although carefully treated in their limited roles as workers and strikers, do not figure prominently in a male-dominated organizational orbit.

Tilly proceeds systematically. She first outlines the historiography concerning Italian socialism that has, among other things, been too purely national in focus, and in the main too glib in its assumptions of political reactions to a standard industrialization experience. She then discusses Italian economic development and Milan's pattern of urban growth. Among the many merits of her presentation are the sensible summaries of major features of Italian economic and social history, capped by clear and persuasive statements about leading trends. Tilly argues cogently that a classic industrial spurt model simply does not apply to Milan or Italy, even during the later 1890s. Having established this background, Tilly moves to a discussion of working-class living standards and the development of increasingly varied organizations. Two interesting chapters assess strike rates, first in two craft sectors and then in the factory branches, noting their relationship to organizational strength but also to government actions such as the liberalization of restrictions on unions in 1901. In the final substantive chapters she turns to the rise of socialism, from early activities and disputes in the 1880s to the formation of the *Partito operaio*. Summaries of major congresses, with attendant material on debates over anarchism and over collaboration with left-wing liberals, lead to Tilly's final assessment of the *Fatti di Maggio* in 1898 and the subsequent, if temporary, opening of the government to greater labor activity.

The tone is frequently descriptive, but Tilly works toward some sensible sociological models of class formation (derived from the work of Aristide Zolberg and Ira Katznelson). At many points she enlivens her presentation by drawing comparisons with other industrial areas, although this is in no sense a systematically comparative work. Internal analysis, while usually succinct, is always clear and almost always persuasive. Tilly thus disposes of any idea that worker action should be explained mainly as a result of immiseration. She analyzes the social basis of the *Fatti di Maggio* in a classic crowd study.

Tilly concludes that the ultimate significance of the rise of the Milanese labor movement lay in its combination of some fairly standard features of organizational/class evolution with the particular constraints of Italian regional diversity and, above all, political repression. The results boiled over at the end of the

1890s in an unusual episode of labor political violence. The outcome had even longer-term ramifications as a working-class tradition was created without adequate organizational base—this being the fruit of state repression. The working class therefore proved unusually vulnerable to attack, as in its collapse in the face of the fascist surge of the early 1920s. Milan here is contrasted not only with France and Britain but also with Germany, where despite government intervention the working-class substructure gained more ample development. Tilly's final reference to fascism requires additional analysis, for she leaps over the intervening twenty years, but it caps her carefully constructed edifice as it leads from halting industrial change to an assertive political voice for labor.

Inevitably in a book that is at once admirably ambitious and commendably succinct, there are some omissions. The experience of work is largely assumed, save for one interesting passage on changes in the organization of machine building. Employers and their policies appear only sporadically. Workers' standard of living is assessed quite clearly, but with few statistics and through discussions of food and housing alone. Perhaps most troubling is the absence of much direct worker voice. Because the book's chapters are more juxtaposed than analytically joined, we are encouraged to believe that socialists came to speak for workers (or, at least, for potentially active workers) without ever seeing that proposition put specifically to the test. Worker evolution leads to politics, which is in turn primarily shaped by the political initiatives of the state; most other findings, including very interesting assessments of distinctive Milanese business structures, recede once these connections emerge. It might have been helpful to gain a fuller sense of the kinds of goals workers themselves brought to this whole equation. In this sense, the very scope of Tilly's sure-handed interpretations opens some additional issues.

PETER N. STEARNS
Carnegie Mellon University

BRUNELLO VIGEZZI. *Politica estera e opinione pubblica in Italia dall'Unità ai giorni nostri: Orientamenti degli studi e prospettive della ricerca.* (Politica Estera e Opinione Pubblica, Quaderni, number 5.) Milan: Jaca. 1991. Pp. 220. L. 22,000.

In Italy as elsewhere, the relationship between public opinion and foreign policy has been an ambiguous and perplexing one. This, of course, can present excellent opportunities for historians to develop a methodology and to explore the dimensions and significance of that relationship. This, in brief, is the subject of Brunello Vigezzi's book. The author begins by pointing out that the tensions of the unification period accustomed Italians to view foreign policy issues as being intimately connected with public opinion. At the same time, however, public opinion has

usually been fragmented into various "opinions" that have made it difficult to visualize a larger national consensus.

Although a number of fine works dealing with individual periods and specific issues have appeared, a comprehensive study covering the period since unification remains to be written. Vigezzi suggests that the absence of "good" studies on the relationship between foreign policy and public opinion in Italy reflects not only methodological difficulties but also the complexity of that ambiguous relationship in Italy. With these concerns and hopes in mind, Vigezzi deems it worthwhile to query those authors who have in some way dealt with the subject, and to explore their approaches, criteria, and results. In his view, the history of Italian foreign policy and public opinion reached its most significant stage during the 1920s and 1930s, with the work of Benedetto Croce, Gioacchino Volpe, Gaetano Salvemini, and Luigi Salvatorelli. Although some would argue that studies on foreign policy written during the interwar years were to varying degrees influenced by fascism, and Vigezzi would be among the first to agree, he does maintain that a number of Italian historians of that era did overcome the immediate influence of fascism and were able to examine the relationships between foreign policy and public opinion *sui generis*, such as Federico Chabod, Walter Maturi, Carlo Morandi, Ernesto Sestan, Nello Rosselli, Rodolfo Mosca, Augusto Torre, Mario Toscano, and Franco Valsecchi. These historians were successful in placing the fascist period in greater historical perspective and in focusing more dispassionately on the relationship between foreign policy and public opinion. Vigezzi has considerable admiration for that generation of Italian historians and would go so far as to maintain that they constituted a "school" that laid the foundations for the study of the history of foreign policy and public opinion in Italy.

This brings Vigezzi back to the more immediate problem: contemporary studies continue to suffer from a lack of direct and focused research that explores the meaning of "public opinion" in the Italian context. What is lacking is a conscious effort to trace the mixed history of the relationship between foreign policy and public opinion from unification to the present so that we might understand the reasons for that "troubled" relationship and its importance for the history of Italy from a more contemporary and perhaps more dispassionate perspective. In that task, the masters of the 1920s and 1930s, he argues, have a great deal to offer. In addition to outlining the scope, significance, and difficulties of the challenge, the author also offers excellent conceptual and methodological possibilities. As such, this is indeed a contribution.

E. ARGENTO
University of British Columbia

PHILIP D. LONGWORTH. *The Making of Eastern Europe*. New York: St. Martin's. 1992. Pp. xii, 320.

Contemporary Eastern Europe: can it be explained by historians? Should historians concentrate on problems of the past or rather ask questions directly related to the present? Philip D. Longworth sees his task as "an enquiry into the factors which shaped Eastern Europe's development and have given it the character it has today." In order "to allow readers who know something about contemporary Eastern Europe but little about its past to start with the familiar and gradually acclimatize themselves to the variety of its peoples, its plethora of place names and the strangeness of its institutions," he has adopted an antichronological order of presentation.

What has been Eastern Europe? Longworth is right when he avoids a formal answer. The reader may conclude that its inhabitants used to behave strangely, or rather differently from the Westerners. Did they? Yes and no. Longworth traces "regulatory measures" of the state in the East to "the very beginning," or the early Middle Ages (p. 286). East Germany's "distinctiveness . . . with its strong military and musical traditions" is in a way connected with the fact that it had not been included in Charlemagne's empire (p. 301). But while Brandenburg and East Prussia were Germany's border areas, Saxony and Thuringia belonged to its medieval core lands. Eastern Europeans tend to confuse politics with morality, we are told, and to regard political leaders as sources of salvation rather than arbiters between conflicting interests and fallible promoters of the "common good." The origins of such attitudes lie reportedly not in the communist period but "in the early Middle Ages when Byzantium was in its prime" (p. 260). But what about charismatic leaders in the West, from Napoleon to Adolf Hitler?

Longworth divides the long period from the foundation of Constantinople (324 A.D.) until 1989 into ten periods; yet his counterclockwise movement—from the post-Stalin era to Emperor Constantine—cannot continue inside each chapter and this disturbs the logics somewhat. Each chapter is concluded by a fine paragraph or two asking questions directed toward the preceding period, but an important question remains: was that violation of chronological routine well substantiated?

As someone who enjoyed this book much more than it may appear from these critical comments, I felt somehow like a passenger in a time machine thrown by turns into forward and reverse gear. Moving deeper and deeper into the past may be a joyful ride for those who are familiar with the material, but frequent references to subsequent chapters may make one question the wisdom of such a construction. It would be more clear and practical had the author started with a set of questions related to our times and continued looking for answers into the Eastern European past, close and distant. In such a

structure, chapters would be thematic, not chronological. As it is now, the present-minded reader, or the political scientist seeking inspiration in history, will see no clear reason why, for instance, the period between Mohács (1526) and White Mountain (1620) is explained before that period begun in 1352 (conquest of Gallipoli by the Ottomans) and closed by 1526. A side effect of the antichronological construction of the text is that some items are explained not when they first appear in the text but rather at the earliest place allowable by the book's structure. For instance, the Arians are banned from Poland on page 164 without any explanation who they were; that information appears on page 184. "Tulipomania" in Istanbul is mentioned no fewer than three times (incidentally, without alluding to its Dutch counterpart), twice without explanation.

A longtime teacher of East European history and frequent visitor to the area, Longworth acquired great experience with the variety and strangeness of the region. He is one of the very few scholars, in the East and in the West, who regard Albania as not too strange to be included into the picture. But even inside its own borders (whatever they were), the area in question has been divided deeply and in many ways. Probably the principal characteristic of that area is its extreme variety: geographic, ethnic, cultural, political. (See J. Beauprêtre, ed., "L'Europe Centrale: Réalité, mythe, en jeu XVIII^e–XX^e siècles," *Les Cahiers de Varsovie*, 22 [1991].) It seems that Eastern Europe cannot be defined if not by opposition to "Europe" or to Western Europe.

A medium-sized monograph on such a wide subject is a challenge to its author's knowledge and skill. A certain number of errors can hardly be avoided. With respect to a potential new edition, some of them may be enumerated here. Berlin was divided by the Wall in August, not in March, 1961 (p. 21). Józef Piłsudski returned to Poland from German prison on November 10, 1918, not in October (p. 65). Matthias Corvinus would have been deeply offended at being called "Matthias the Crow"; in 1572 Jan Zamoyski was not yet chancellor (p. 186); the Wiśniowieckis were not Lithuanian magnates (p. 195), but Ruthenian or Ukrainian. Incidentally, for the reader unfamiliar with East European subtleties, the puzzle is not solved: what is the relationship between terms like Ruthenia and Ukraine (yet the Kiev-Moscow relationship is explained well)? The list of religious orders instrumental in the triumph of the Counter Reformation is too long: the Augustinian Order had been almost destroyed in the times of Reformation; Benedictines and Cistercians had exhausted their energy in the Middle Ages, and Paulines kept but a few houses in Hungary and Poland.

Economic questions receive little attention, but they are well covered. Some doubts may be raised by Longworth's explanations of serfdom. "Second serfdom," a term coined by Friedrich Engels, is not necessarily a wrong one. The earlier serfdom (in

Poland connected with *ius ducale*) withdrew when, from around the thirteenth century, the German Law was introduced and the rents were commuted. Incidentally, serfdom is somehow mixed up with labor rent (p. 102): in the Kingdom of Poland serfdom had been abolished already by Napoleon, while land reform (ownership of land to the peasants) was introduced only in the wake of the 1863 insurrection. Finally, the dissolution of landed estates in Poland after World War II is largely misrepresented (p. 58).

A few arguments seem open to discussion. One may wonder whether in 1944–45 "a conciliatory attitude [of the Polish government in exile] similar to that of the Finns . . . might have preserved a similar degree under a non-Communist government" (p. 46). But what about President Edvard Beneš and the communist "victorious February" of 1948? Even if we leave aside this counterfactual problem, Longworth serves us another one: Poland's "refusal to countenance" access for Soviet troops to eastern Germany across its territory "in 1939 had arguably precipitated the Nazi-Soviet pact" (p. 46). In chapter 3, the accusatory argument is pushed much further. In 1939, the Soviets were "ready to defend Poland by an effective show of force, and even to fight a short war in her defence, [but] Moscow was not prepared to make a non-reciprocal commitment." Longworth does not say what kind of reciprocity they expected from Poland. "According to this reading of events Colonel Beck was himself responsible for the conclusion of the Nazi-Soviet pact" (p. 88). For all the mistakes committed by the Polish government and its foreign minister in the late 1930s, the government did not share Longworth's opinion that Stalin was ready to defend Poland, and it understood well what Soviet army bases in Poland would mean. The counterclockwise presentation of facts should have offered the author relevant analogies from Baltic states in Eastern Europe to post-World War II Africa or Afghanistan.

In many other cases Longworth points with gusto at historical analogies over a long-term time span, real and fictitious, and tries to uproot errors of the conventional wisdom. My comments concentrate chiefly on the last (that is, the earliest) chapters. They make very good reading.

ANTONI MACZAK
University of Warsaw

GERSHON DAVID HUNDERT. *The Jews in a Polish Private Town: The Case of Opatów in the Eighteenth Century*. (Johns Hopkins Jewish Studies.) Baltimore: Johns Hopkins University Press. 1992. Pp. xvi, 242. \$39.95.

Gershon David Hundert's study offers a prime example of the renaissance of Polish-Jewish historiography that has taken place in the 1980s and 1990s. This case study of Opatów (known in the Yiddish of Polish Jewry as Apt) is an *Alltagsgeschichte* thoroughly

grounded in archival research and empirical measurement. But if Hundert's purpose consists primarily of reconstructing the economic, political, legal, and cultural workings of the Jewish community, he also takes the opportunity to look afresh at larger, interpretive issues. Among these are the Jewish role in the urbanization of Poland-Lithuania, the purported domination by Jews of Polish commerce, the scope and nature of Jewish-magnate ties, relations between Jews and non-Jews, and the question of long-term economic and social decline in the eighteenth century.

The central and most controversial issue in this list concerns the unique relationship that evolved between the Jews of Poland-Lithuania and the nobility (*szlachta*), or, more properly, that segment of the landowning nobility that held most of the wealth and power, the magnates. Hundert reminds us that more than half of the Jews in the world in the eighteenth century lived in Poland-Lithuania and that about one quarter of all the Jews in the world lived in Polish private towns like Opatów. He expresses skepticism (as has Murray Rosman before him) at the conventional wisdoms of both Polish and Jewish historiography, which have held that the nobility manipulated the Jews in order to achieve economic and political control over other segments of Polish society, that the magnates dealt in a capricious and arbitrary manner toward the Jews, and that relations between Jews and Polish peasants became poisoned as a consequence of Jewish implication in the exploitative feudal system.

Hundert argues that the behavior of the magnate families who owned Opatów on the whole was not capricious. Although there could be no doubt as to where power ultimately lay (even the much-touted autonomy of the Jewish community was seen by the magnate as an extension of his personal authority), relations between magnates and Jews operated normally on the basis of law, custom, and negotiation. That the magnates generally supported—and helped to legitimate—the institutions of Jewish communal autonomy is illustrated by the instruction issued by Paweł Karol Sanguszek in 1737 that appeals cases coming from the Jewish court be adjudicated on the basis of "Jewish law and custom" (p. 137). Thus, if the authority of the rabbis rested ultimately on the will of the Polish lord, it was also the case that in the lord's court the will of the rabbis could be heard. The political-institutional relationship between the magnate and the Jewish community, then, was symbiotic as long as the magnate wished it to be.

In contrast to the view that Polish feudalism rested on a closed system of exchange that punished productivity and the creation of surplus, Hundert suggests that the noble owners of Polish private towns were economic rationalists of a sort who sought to maximize trade both within their lands and abroad. And as "absolute monarchs" on their own domains they often turned a deaf ear to the petitions of the economic antagonists of the Jews, with their appeals

to custom and precedent. In 1708, the Christian burghers of Opatów complained to the lord's commissioners that the Jews were violating city law by producing beer and mead, and selling wine, hay, oats, herring, "even pork, which they do not eat," outside of their own street. The commissioners countered that the Jews enjoyed privileges of their own and had the right "to sell anything anywhere on any day" (p. 135).

Over the course of the eighteenth century the magnate owners of Opatów, Paweł Karol Sanguszek and Antoni Lubomirski, intervened increasingly in the affairs of the community in matters ranging from elections of *kahal* elders to taxation to the selection of rabbis. Lubomirski in particular took a direct and personal interest in all aspects of communal finances, issuing numerous instructions concerning the collection of various taxes and taking care to insure that his directives were obeyed. His interest in the affairs of the Jewish community extended literally to the movement of individuals. Fearful that wealth was moving out of the town, he ordered two fathers-in-law in 1776 to bring their sons-in-law back to Opatów within two weeks "on pain of a huge fine" (p. 152). Yet Jews could and did leave Opatów, and it was their ability to remove themselves and their talents from the town that defined the limits of the magnate's power. As Hundert concludes, "Jews put their commercial, industrial, and managerial expertise in the service of the aristocrat in return for the peace and security and good order that he provided" (p. 153).

This is a most valuable study, thoughtful, rich in detail, and peppered with insights. Hundert is unusually adept at describing the day-to-day functioning of the Jewish community, portraying the lines of authority, power, and prestige, and highlighting the role of a small, oligarchic network of families that operated both within Opatów and in Poland at large. Most impressive, to my mind, is his ability to explain Jewish autonomy from the perspective of the natural history of Jewish-magnate relations.

HILLEL J. KIEVAL
University of Washington

JAMES FORSYTH. *A History of the Peoples of Siberia: Russia's North Asian Colony 1581–1990*. New York: Cambridge University Press. 1992. Pp. xx, 455. \$79.95.

For nearly two hundred years, historians of Siberia dealt mainly with the Russian conquest. Absorbed by the scope, color, and drama of their subject, they said little about the region's history after the conquest, and least of all about the fate of the indigenous peoples. Soviet historians attempted wider coverage but had to work within a conceptual framework of class conflict, antireligious dogma, "the friendship of peoples," "the elder brother" helping the younger brother, progression toward ever higher stages, and the priority of "building socialism." Now, at last,

James Forsyth has turned the history of Siberia inside out, writing from the point of view of the peoples overrun by the Russians.

Forsyth deals first with the Khantys and Mansis, the Samoyeds, the Selkups and Kets, the Siberian Turks, and other peoples in the path of the initial advance, until the end of the sixteenth century. He then takes up those peoples in the path of the advance to the Urals and into the Ob River valley, and after that into central and eastern Siberia (the Yakuts, Tungus, Buriats, Yukagirs, and last of all the Chukchi). Beginning with Vitus Bering, there is a short section on the next phase, the takeover of the Aleuts and certain other peoples of what became known as Russian America.

This is a large order, but Forsyth has combed the abundant yet widely scattered literature with good result. He traces the fate of the peoples involved until the nineteenth century, and then through the period of accelerating colonization. The last quarter of the book deals with the Soviet period. Following the doctrine of self-determination, the new Soviet state was divided on the basis of ethnicity. During the 1920s, the so-called Committee of the North tried to help the Siberian peoples to retain their traditional ways, barring Russians from making any use of the natives' territories. This benevolent policy was set aside during the first Five Year Plan, to be replaced with collectivization of herds, joint farming, and the creation of organs of government for the tribes that would be congruent with those of the rest of the country, as well as an attempt to apply the principles of class struggle and advocacy of the abandonment of nomadism. These policies ruined the region's reindeer herding economy and swamped the smaller aboriginal peoples with an influx of outsiders brought in by great industrial complexes that sprang up beside traditional settlements.

With perestroika the last great colonial empire fell apart, but not completely. Although the larger units separated, the core area, the giant RSFSR, now "a firmly Russian land," remained intact, and its small indigenous peoples seem certain to remain under Russian rule.

Forsyth's concluding chapter, "Siberia in the 1980s," is based on the surge of new information made available under glasnost. Like many of their North American counterparts, the non-Russian peoples of Siberia suffer from bad housing, unemployment, alcoholism, loss of traditional cultures, and low life expectancy. A high birthrate has stabilized the ratio of indigenes to Russians at about 5 percent to 95 percent, but there are many single-parent families, and the native population is largely deprived of its culture, clan divisions, and ethnic consciousness.

Forsyth urges that the indigenous peoples of Siberia be assured that their lands have ceased to be colonies exploited by Russian rulers. In his view they must be given a form of statehood in which their sovereign rights are recognized and sufficient auton-

omy so that they can influence industrial and social policy. This seems possible for the Yakuts and Buriats, who are numerous enough to demand special consideration, but for the smaller peoples it seems unlikely that these laudable aims will be achieved. Although there is rising consciousness in many countries about the fate of smaller peoples, world-wide trends of expanding population and developing technology will doom many to extinction, or at best assimilation. In Siberia, as in Alaska and Canada, the snowmobile, the dish antenna, modern habitations, store-bought foods, and other peoples' languages seem likely to prevail over traditional cultures.

RICHARD A. PIERCE
University of Alaska,
Fairbanks

EDITH W. CLOWES *et al.*, editors. *Between Tsar and People: Educated Society and the Quest for Public Identity in Late Imperial Russia*. Princeton: Princeton University Press. 1991. Pp. ix, 383. Cloth \$59.50, paper \$16.95.

This volume is a collection of twenty essays on the emergent "middling" groups or strata in late-imperial Russia. The editors' ambition is to study the relationship between social change and new patterns of social identity (the emergence of a well-defined private sphere, the assertion of individualism on various fronts, the formation of a civic society, the appearance of new attitudes toward religion and the state, the reinterpretation of social collectives and of the national destiny). To achieve that lofty objective, the editors have called on social historians, literary critics, art historians, and students of Russian architecture and the press. The salutary result is a book that opens to debate questions such as the existence in Russia of a middle class, the hegemony of "middle-class values," and the general direction of Russia's social evolution before the October Revolution.

The editors suggest that historians should be wary of employing traditional terms to denote the middling strata, for labels such as "bourgeoisie," "middle class," and even "educated public" may be "slippery," "elusive," or plainly "unsuitable to the Russian case" (pp. 3-14). The editors' caution seems justified in view of Abbott Gleason's careful historical taxonomy of the meanings of the Russian words *burzhuaizii* (bourgeoisie) and *obshchestvo* (educated public), and in view of several essays that deny the cohesion of Russia's middling groups before World War I. For example, Thomas C. Owen's essay on "Impediments to a Bourgeois Consciousness in Russia" asserts that "the Russian corporate elite failed to achieve even a semblance of unity in formulating economic strategies and political attitudes, much less a mature class consciousness" (p. 76). He sees the elite divided along ethnic, social, cultural, and regional lines. According to Harley Balzer, Russian professionals were increas-

ingly diverse in the decade before 1917; he thinks their diversity illustrates "the extent of social fragmentation in tsarist Russia" (p. 197). The school teachers studied by Christine Ruane and Ben Eklof had "no uniform social portrait" (p. 201). Although in 1905 many teachers joined the union movement, their unity quickly disintegrated over social and ideological differences. This kind of social fragmentation apparently prompted Alfred Rieber to speak of the "vast splintered middle of Russian society" (p. 356).

If the middling strata lacked social cohesion, they nevertheless attempted to overcome their amorphousness by adopting certain political or cultural stratagems. James L. West ably demonstrates that Moscow industrialists in the Riabushinsky circle unsuccessfully tried to transform the old merchantry into a modern bourgeoisie. John Norman and John Bowlt analyze merchant patronage in the Moscow art market. Norman shows that P. M. Tretiakov considered his marvelous art collection to be one merchant's contribution to public enlightenment; meanwhile, Bowlt sees art patronage in the period as a manifestation of "middle-class values," even though he is quick to point out that Moscow patrons of the Silver Age were not "a single class united in their aspirations and endeavors" (pp. 108–09). Joseph Bradley's study of Moscow's voluntary associations stresses the achievement of postreform Muscovites in creating unofficial groups where entrepreneurial, professional, and cultural elites could meet, although he readily admits that "civic society remained 'gelatinous' and brittle" (p. 148). William Wagner's article on divorce law reform suggests that certain "middle-class" notions—individualism, commitment to a society based on merit and legality—had found a constituency by century's turn, but that not every member of the putative middle class adhered to such views. The failure to discover an appealing "middle-class" ideological banner for urban intellectuals and the lower middle strata is the subject of two remarkable essays, one by Daniel Orlovsky on postal and white-collar workers in 1917, the other by Gregory Freeze on the Orthodox church's "urban mission" to intellectuals. In a major revision of historiography on the October Revolution, Orlovsky contends that lowly white-collar workers joined the Bolshevik cause in large numbers after midsummer 1917. The so-called "proletarian revolution" was by no means a purely proletarian phenomenon. Freeze brilliantly shows that the church tried to win the liberal public's allegiance but failed to do so because of repression from above, the clergy's isolation, its discomfort with secular ideology, and its preoccupation with religion in rural areas. The failed link between religion and the bourgeoisie is also a topic treated in Bernice Rosenthal's essay on Sergei Bulgakov's fascinating attempt to define a Russian Orthodox work ethic in contrast to Max Weber's theory about the Protestant ethic as the mainspring of capitalism.

Russian belletrists, artists, and architects also re-

sponded to the middling strata's social dynamism. Edith Clowes reminds us that the Moscow Art Theater staged numerous plays about the rising merchant class and declining gentry, but she observes that nowhere in these productions did the middle of Russian society find a strong protagonist. James Curtis makes a (weak) case that the painter Konstantin Korovin's treatment of public and private space is an indicator of Russia's ongoing process of becoming bourgeois, while William Brumfield argues, on better authority, that many of Moscow's architects intended *style moderne* buildings to create a new environment for Russia's middle strata. Still, one has the impression that the artistic and literary world was, by and large, hostile to the middling strata. Members of Maxim Gorky's *Sreda* circle, described in a lively essay by Mary Louise Loe, rejected bourgeois conformity, underscored the isolation of the intelligentsia from the common people, and to some degree personified the pervasive sense of illegitimacy felt by so many in Russia's middling strata. As Louise McReynolds notes, even the great journalist V. M. Doroshevich, who attracted so many readers from the lower middle strata of society, ultimately welcomed the October Revolution.

As my partial survey suggests, this is a rich and detailed book. My only regret is that none of its contributors satisfactorily answers the theoretical question of whether one should speak of "middle-class values" or "middle-class ideology" in the absence of a sociologically coherent middle class. To say that before 1917 a middle class was emerging from estate society, as several essayists do, is to assume what must be proven. Another essayist incautiously equates "bourgeoisie" and "educated society," while simultaneously admitting that "there was no real middle class, only a growing agglomeration of middling groups, divided by ethnic and cultural differences that were bridged only rarely and with difficulty" (pp. 30, 34). One of the editors attempts to see Russia's new "public culture" or "public sphere" as somehow comparable to imperial Germany's bourgeois *Öffentlichkeit*, an attempt that, even after a page of qualifications, strikes me as more than dubious. Still, at this stage, theoretical muddle is much to be preferred to the virtual silence on such matters that reigned for so long. The editors and authors of this volume should be congratulated for their very impressive achievement.

G. M. HAMBURG
University of Notre Dame

TIMOTHY EDWARD O'CONNOR. *The Engineer of Revolution: L. B. Krasin and the Bolsheviks, 1870–1926*. Boulder, Colo.: Westview. 1992. Pp. xix, 322. \$55.00.

This book is another of Timothy Edward O'Connor's biographies of leading Bolsheviks (he has also written volumes on A. V. Lunacharskii and G. V. Chicherin).

O'Connor brings to light new material from Russian and European archives and explores the attractions of Bolshevism to the technical intelligentsia and professional engineers as well as L. S. Krasin's own role as a revolutionary and prominent Soviet official (the head of crucial trade delegations in England, Scandinavia, and Germany, and as commissar of Foreign Trade, Communications, and the like) during the earliest years of Soviet power. O'Connor portrays Krasin as first of all an engineer and international deal maker (here he faced many problems similar to today's) and only secondly as a socialist. According to O'Connor, Krasin in his years of primacy (roughly 1918–22) advocated Soviet technocracy, not party rule, and Westernization in foreign policy as opposed to socialism in one country or any version of revolutionary internationalism. On the first point, Krasin maintained that the state could not develop Russia's economy if the party leadership consisted solely of "journalists and literary intelligentsia." Krasin thus was ahead of his time in advocating the kind of merger of party and technocracy that came to dominate the Soviet scene in the 1930s (as noted by Kendall Bailes). Krasin's "westernization in foreign policy" occupies a good portion of the book. Krasin obsessively pursued the state's foreign trade monopoly against all sorts of opposition from charismatic party leaders (N. I. Bukharin), functionaries (G. I. Sokol'nikov), and ideologues. Krasin believed in building socialism through rational economic planning (for him NEP meant a planned economy) and well understood that to deny the monopoly would open the door to the chaotic and self-serving competition among many rival Soviet institutions. No lover of the pure market, he argued that under such conditions Western government and business would bleed the country dry. Yet he doggedly pursued Western capital and contacts (witness the Anglo-Soviet Trade Agreement and German-Soviet Commercial Accord of 1921) and believed, naively perhaps, that stable diplomacy based on commerce (he was the joint venture pioneer) was possible with the West. O'Connor does a good job illustrating Soviet structural disadvantages in trade with the West (lack of refrigeration or of an ocean-going commercial fleet, forcing the Russians to sell quickly and cheaply).

In the end Krasin was not ideological enough, which should tell us something about the ostensible pluralism of NEP, and he lost even Lenin's support at the Genoa Conference, was attacked at the Twelfth and Thirteenth Party Congresses, and was shunted aside before his relatively early death from leukemia in 1926.

O'Connor does his best to bring Krasin to life, but it is not easy given the limitations of the sources and the author's tendency to reproduce (especially in the early chapters) the stiff hagiographic language of Soviet-style biography itself. We read all too often of Krasin's "prodigious energy and organizational talent" (p. 131), for example, or of his tact and indepen-

dence. The book is poorly organized. There are frequent references to his many love affairs, but the issue is not explained until rather late in the volume. The chronology of events and issues is sometimes unclear. O'Connor provides valuable material on Krasin's early years as student activist and professional engineer. He is less successful at explaining the major turning points in Krasin's life, such as his motivation for becoming a socialist, his return to the Bolshevik fold in 1917–18, or his much marveled-at role in the embalming of Lenin's body and sanctification of the cult in 1924. He rejects the accepted notion that Krasin was a closet god-builder and claims weakly instead that Krasin simply believed in the technological mastery of nature. Finally, he largely passes over Krasin's profound debate with Lenin on bureaucracy in 1923 (pp. 224–25) that centered on the rival concepts of control (Lenin) versus production (Krasin). O'Connor sees Krasin's critique of an enlarged *Worker-Peasant Inspectorate* (*Rabkrin*) motivated by fear of inadequate and unprofessional functionaries interfering in economic production and management. True enough, but Krasin was launching a major attack on the very administrative culture promoted by Lenin that had come to dominate the Soviet party/state. Krasin wanted spontaneity of human activity within the framework of the planned economy, whereas Lenin advocated control of the bureaucracy through more and more new layers of bureaucracy. Krasin correctly understood that such a system would destroy human motivation and hence production. No wonder then, that Krasin never was welcomed into the inner circle of party leadership.

DANIEL ORLOVSKY

Southern Methodist University

CHARTERS WYNN. *Workers, Strikes, and Pogroms: The Donbass-Dnepr Bend in Late Imperial Russia, 1870–1905*. Princeton: Princeton University Press. 1992. Pp. 289. \$39.50.

The Russian working class has been a fruitful subject of analysis over the last twenty-five years, about which new data and conceptualizations concerning working-class formation and the revolutionary process in Russian history have been produced. Charters Wynn makes an important conceptual and substantive contribution to this literature with his study of workers and labor unrest in south Russia before and during the 1905 revolution. Focusing on semiskilled and unskilled male workers, the predominant source of labor in this region, Wynn paints a picture of a worker culture shaped by transience, wretched social and economic conditions, and ethnic fragmentation, and a political landscape dominated by drinking, violence, strikes, and pogroms. The book challenges conventional paradigms dominated by images of skilled, Social-Democratic workers and organized, "conscious" revolutionary behavior. Such workers ex-

isted in south Russia, especially in the larger cities, but their ability to organize others was limited by the volatility of the mass of miners and semiskilled workers, who repeatedly joined strikes and labor protest but who just as often reacted to the failure of these actions with pogroms and violence directed at Jewish shopkeepers and artisans.

This is the second solid study to appear on workers in south Russia in the last few years. Theodore Friedgut's book, *Iuzovka and Revolution: Life and Work in Russia's Donbass, 1869–1924* (1989), focused closely on the city of Iuzovka (now Donetsk). Wynn looks at the broader industrial-mining region of the Donbass-Dnepr Bend, including the older industrial cities of Ekaterinoslav and Lugansk, the company town Iuzovka, and scores of small mining villages scattered throughout the formerly sparsely populated region. Wynn relies on a broad range of published documentary sources as well as police, ministerial, and judicial archives. Friedgut's study was based on similar published materials, supplemented by local archives. The two works complement each other and together enrich our understanding of Russian working-class history.

Wynn's study follows a conventional social/political history narrative. He describes the industrial expansion of southern Russia and the peculiarities of its labor force. A superb chapter on working-class daily life depicts a culture of alcohol, gambling, and ritualized, brutal violence, showing quite convincingly the extent to which popular violence was a critical element of Russian working-class culture. (The culture was militantly masculine, as well; women figure as prostitutes; there were few inducements to family life.)

This culture of violence inevitably influenced labor protest in the south, and Wynn chronicles episodes of labor unrest accompanied by destructive rioting, looting, and physical assaults. Paradigmatic of these episodes in the south was a strike in Briansk in 1898. Although Social-Democratic activists on the scene tried to channel workers' economic grievances in a "conscious" and peaceful direction, these strikers responded with violence, first directed against the symbols of the factory regime, and then to the surrounding town, where the crowd attacked liquor stores and shops owned by Jews. Wynn meticulously traces the dynamics of this and other similar strikes and riots. The dynamics become more complicated with the growth of underground revolutionary activism. At times the story becomes a little confusing, but Wynn generally does a good job of integrating the efforts of Social-Democratic, Socialist-Revolutionary, and anarchist activists with the less choate logic of spontaneous unrest.

These activists tended to believe, and Wynn agrees, that the pogroms resulted from workers' frustrations over failed strikes, combined with the prevalence of violence in the culture and the deep-seated ethnic hostility (anti-Semitism) of local workers. But sometimes pogroms and strikes went hand in hand, al-

though occasionally pogroms did not ensue, even after the great frustration of the failed December 1905 general strike. The book ends abruptly here, leaving readers to wonder (and other historians to discover) whether these patterns continued into the Duma period or whether new political configurations changed the pattern of labor unrest in south Russia.

Why is the violent labor history of the Donbass region so different from the story told about St. Petersburg and Moscow? Wynn adduces some factors in an exceptionally strong conclusion. The prevailing transience of the labor force and boom-town conditions meant that it was easier for workers to switch jobs than to fight injustice, undermining workplace cohesion. Social conditions (housing, sanitation, and leisure) were more demoralizing here on the frontier. Police and industrialists cooperated more closely. And there was a large indigenous Jewish population providing convenient scapegoats for Russian workers' frustration and rage. This study raises important questions about working-class culture, social determinants of political behavior, and the adequacy of our notions of the polarities "revolutionary/reactionary" and "spontaneity/organization." It makes an important contribution to Russian social history and to comparative working-class history and deserves a wide audience.

DIANE P. KOENKER
University of Illinois,
Urbana-Champaign

JONATHAN COOPERSMITH. *The Electrification of Russia, 1880–1926*. Ithaca: Cornell University Press. 1992. Pp. xii, 274. \$39.95.

Electricity in this century has attracted both engineers and dreamers, and the Russian encounter with this new technology has been exemplary. Jonathan Coopersmith's study of Russian electrification from the late tsarist period to the first decade of the Soviet regime examines both sides to the subject. He himself is sympathetic to the dreamers. Their great moment came in 1920, when Lenin's revolutionary government raised electricity to the status of public good. The slogan "Communism is Soviet power plus the electrification of the whole country" defined a dream of the future with roots in the Russian past. Coopersmith has written a careful account of that past and of the NEP years when that dream lost its glitter.

Coopersmith is critical of Russia's prerevolutionary experience with electric power. The first half of his book traces the sporadic involvement of state agencies in promoting electrification, the major role in that process of foreign companies and technology, and the rise by the turn of the century of an influential group of Russian electrical engineers. As in the West, the principal centers for the spread of electric power were cities, but the results were disappointing. Until

the last years before World War I, electric power grew slowly by comparison with Western Europe.

Coopersmith holds the tsarist government responsible for this situation. He believes that it placed bureaucratic obstacles in the way of electrification and generally was unsympathetic to the new technology. The real problem, however, behind the slow pace of Russian electrification was, in my opinion, economic backwardness. The author subscribes to Alexander Gerschenkron's thesis of the demand-side weakness of the Russian economy, in need of massive government purchases to spur industrialization. But more recent work by economic historians such as Fred Carstensen have uncovered crucial supply-side factors retarding growth. One of these appears frequently in Coopersmith's own discussion, namely, the woeful shortage of Russian credit for long-term investment. In this light, active Western involvement made good economic sense.

World War I began the process of direct state intervention in electric power operations, culminating in Soviet control of electrification. The author points to a few visionary Russian engineers who before the war held out prospects of regional electric power networks covering the entire country. Only the war, however, moved the tsarist government to look to the needs of the whole country. By the late war years, public agencies had become deeply involved in assuring electricity to industry and the cities in what Coopersmith terms the "trend toward electrification as a state technology" (p. 125).

The last half of Coopersmith's study traces the history of electrification and state policy under Soviet rule. In broad outlines the story is a familiar one, but the author explores in detail both the visions that electric power aroused among communist leaders and the grim realities of economic destruction and impoverishment that they confronted. He devotes an entire chapter to the establishment in 1920 of the State Commission for the Electrification of Russia (GOELRO), and to the discussion by the eighth congress of Soviets of its plan for a state network of regional power stations. The apotheosis of this revolutionary dream came when in their darkened hall the delegates to the congress contemplated an illuminated map of the future electrified Russia.

The results, as Coopersmith makes abundantly clear, could not measure up to the dream. His study points by way of explanation to the economic shortages that stood in the way of this plan, and to the new direction of party economic policy that by the late 1920s placed highest priority on heavy industry. As a result, electric power came to be viewed as a scarce resource reserved primarily for major industrial and urban centers. Stalin never appeared in propaganda posters holding a light bulb.

This study has an important story to tell about the reception of technology in tsarist and communist Russia. It is not an easy read, for the style is at times awkward and details tend to overwhelm the analysis.

Still, its history of Russian electrification opens a revealing perspective on Russia's economic development and on the revolutionary dreams that once inspired the Communist leadership. I recommend it for anyone interested in Russian economic history and the comparative history of technology.

DANIEL BROWER
University of California,
Davis

V. V. SHELOKHAEV. *Ideologiia i politicheskaiia organizatsiia rossiiskoi liberal'noi burzhuazii 1907–1914 gg.* [Ideology and the Political Organization of the Russian Liberal Bourgeoisie, 1907–1914]. Moscow: Nauka. 1991. Pp. 231. 6 r.

The Moscow historian V. V. Shelokhaev is well known as the leading Soviet specialist on the early history of the Constitutional-Democratic Party (the Kadets) and of the Union of October 17 (the Octobrists). His special forte has been investigation of the organizational structures and membership characteristics of the two main pro-constitutionalist political parties to emerge in the course of the revolution of 1905, the radical-liberal Kadets and the conservative-liberal Octobrists.

In two earlier works, one for each party, Shelokhaev carried the investigation from the foundation of the parties in the autumn of 1905 up to Petr Stolypin's "coup d'état" of June 3, 1907. Here he continues the investigation of organized Russian liberalism or constitutionalism ("the liberal bourgeoisie") up to the outbreak of World War I. In organizational terms, it is the story, already fairly well known in general outline but buttressed here with some new quantitative information from party archives, of the progressive atrophy of the once considerable local Kadet party groups, and of the rapid decline and collapse of the organizationally weaker Octobrist Party, whose firmly pro-constitutionalist elements contributed along with right-wing Kadet dropouts to the rise of the Progressist Party in the context of the Fourth Duma (1912–17).

The first chapter of Shelokhaev's book is devoted to the parties' organization and membership characteristics (size and social background). Following chapters deal with matters of doctrine and tactics, reform platforms, economic programs, positions on the national question (unitary state versus federalism versus national-minority independence), and, as the war approached, foreign policy issues from the party perspectives.

Shelokhaev is at his best on the issues when describing the ins and outs of internal Kadet Party debates, retrieved from the unpublished minutes of Central Committee meetings. Mastery of the sources and issues is less impressive vis-à-vis the Octobrists and Progressists, and the Duma politics of all the parties are treated on the whole superficially; but for that we

have the work of A. Ia. Avrekh, V. S. Diakin, and others.

From the perspective of 1993, the conceptual-explanatory framework of this book, turned over to the publisher in the autumn of 1990 and published in 1991, seems shockingly dated. It is in fact a perfect example of pre-perestroika academic Soviet historiography. In that tradition, getting anything published on the "non-proletarian" political parties (read: everybody but the Bolsheviks) was a progressive act in itself. All one needed by way of conceptualization was a few quotations from Lenin's sarcastic contemporary diatribes about the "class-bound," "fundamentally reactionary," and "anti-revolutionary essence" of the lot of them; generally speaking, the more innovative the project, the more likely it was to rely exclusively on "the Leninist heritage" in lieu of interpretation. And so it is with this book.

Glasnost has definitely caught up with the professional historians of the late-Soviet period. When will we begin to see the results of conceptual perestroika?

TERENCE EMMONS
Stanford University

R. W. DAVIES, editor. *From Tsarism to the New Economic Policy: Continuity and Change in the Economy of the USSR*. Ithaca: Cornell University Press. 1991. Pp. xx, 417. Cloth \$57.60, paper \$18.95.

Continuity and change as an integrating theme for research on late imperial Russia and the ensuing Soviet era is commonplace. R. W. Davies acknowledges as much in his introduction to this important book. But he also aptly notes that few historians actually conduct research on both periods, which is not too surprising given the traditional treatment of the revolution of 1917 as a watershed in political, economic, and social terms, and, more significantly, the real differences in the primary source materials available and the methodologies appropriate to their use. The contributors to this volume have been given the daunting task of rectifying what is indeed a major research gap in Russian and Soviet studies, the analysis of change in key economic sectors across this traditional watershed.

The overriding goal of this collective research endeavor is to garner evidence that can be used to assess the essential features of the economic systems prevalent before and after the revolution of 1917. If it could be established, for example, that the agricultural and industrial sectors were "developing satisfactorily" during the final years of the imperial era, and that recovery from world and civil war was evident during the mixed economy of the New Economic Policy (NEP) of the 1920s, then, as Davies observes, "the stubborn retention of power by the Bolsheviks may in itself have been responsible for the tyrannical political and economic system which was imposed in the Stalin period" (p. 1). Although the challenge to

the fourteen contributors to this volume is clear enough, the nature of the evidence assembled and the conclusions reached are rather more ambiguous.

In most cases, data for 1913 and the "economic year" 1926–27 are used in the analyses of key economic sectors. On occasion data for longer periods are averaged in order to dampen the impact of aberrant events, such as the exceptional harvest of 1913 owing to unusually favorable weather. Part of the difficulty in sorting out what progress, if any, was made during the mixed economy years of the NEP has to do with the very nature and underlying assumptions of contemporary statistical data. For example, conventional wisdom has it that the Soviet economy had been restored to the output levels of 1913 by 1926–27. Although many of the contributors corroborate the recovery in physical output terms, they also make plain that there was much that was not auspicious about the nature of the economic system the Soviets had put in place.

The analyses and interpretations provided in this book are organized into five parts. The first sets the social context. Maureen Perrie and Davies examine principally the urban social class change produced by the revolution, covering ground seldom trod by others. The consequences for the peasantry of the revolution are carefully analyzed by Stephan Merl, who concludes there is precious little evidence to support the notion of the emergence of a capitalist element in agriculture during the 1920s. Indeed, according to Merl's data, rural households continued to rely almost exclusively on family labor right down to collectivization.

Part 2 assesses the relationship between agriculture and economic restructuring. In separate chapters by S. G. Wheatcroft and Mark Harrison on the central question of the relative contribution of agriculture to industrialization before and after the revolution, there is still ample room for debate. Did peasants in the 1920s market less because lower taxes required them to sell less? Or did they sell less because they would receive less in manufactured goods? There is evidence in this volume to support both positions. Indeed, one of the many strengths of this book is precisely that the arguments put forward by the authors do not always align.

Parts 3 and 4 discuss the main industrial sectors, the railways, and foreign trade. One of the most interesting chapters is that by J. M. Cooper and R. A. Lewis on research and development. It underscores the basic point in another chapter by Peter Gatrell and Davies that the labor productivity gap between Russia and the Soviet Union and the Western industrial economies was at least as great at the end of the 1920s as it was in 1913. That comparatively little by way of domestic research and development was supported before or after the revolution unquestionably played a part in what could only be perceived as a distinctly worrying state of affairs from the military point of view.

In the final part, Paul Gregory revisits the vexing issue of national income accounts, price deflators, and the position of Russia in 1913 compared to the Soviet Union in 1928. It is likely that government officials of the 1920s consciously selected a methodology that best fit the expectations of government leaders in terms of rate of economic growth, a scarcely novel situation in our own times. Gregory presents a compelling argument that per capita income in 1928 was still below that of 1913, notwithstanding the real gains of the NEP period and in contrast to public proclamations of Soviet officials. Whether these officials were cognizant of the real situation and the deficiencies of the economic system detailed in much of this book remains a moot point.

This volume sets forth an important and ambitious agenda, and it goes a long way indeed toward fulfilling it. For those interested in the open questions addressed, the evidence and arguments assembled by the contributors will prove invaluable. In fact, the close to one hundred pages of statistical tables are alone worth the price of the book.

JAMES H. BATER
University of Waterloo

EDITH ROGOVIN FRANKEL *et al.*, editors. *Revolution in Russia: Reassessments of 1917*. New York: Cambridge University Press. 1992. Pp. xx, 434. Cloth \$59.95, paper \$18.95.

This volume of essays, the outcome of a conference in Jerusalem in 1988, was conceived by its editors as a representative sample of Western research on the Russian Revolution. The book is devoted to Israel Getzler, a distinguished Israeli scholar and a biographer of Iulii Martov. The volume indeed presents a number of contrasting views and interpretations.

For Getzler the revolution of 1917 released a great potential of democratization. The soviets became schools of democracy, whereby elaborate procedures of election and recall involved hundreds of thousands of people in the process of government. Until September the soviets were led by the Mensheviks and SRs, and they were the architects of democratization. The slogan "all power to the soviets," Getzler argues, was not directed against the Constituent Assembly. The Bolsheviks, on seizing power in the name of the soviets, destroyed them and turned them into bureaucratic organs of a centralized state. Getzler sees the Bolsheviks as having hijacked a people's revolution.

By contrast, most of the authors in the first two parts of this book present a classic revisionist view of 1917. According to Donald Raleigh, soviet power was de facto established in 1917 in many provinces and cities. Naturally and inevitably, the revolutionary process led to the Bolshevik assumption of leadership of the soviets; and the revolutionary masses enthusiastically supported the Bolsheviks. Initiative from below is an important ingredient of revisionist thinking. Rex

Wade provides a picture of the masses outdoing the Bolsheviks in their revolutionary ardor: they practically force the Bolshevik party into action. In the process of escalating class struggle with the bourgeoisie, the workers developed proletarian consciousness. William Rosenberg discusses proletarian identity, class formation, and class struggle. Ziva Galili depicts the bourgeoisie as becoming increasingly unwilling to compromise. In the end, the masses make the October Revolution. The Bolsheviks simply come to power. There was no putsch, no seizure of power is mentioned, no plotting behind the back of the Second Congress of Soviets nor plans to disband the Constituent Assembly. We have here essentially a Marxist-Leninist interpretation of 1917. Allan Wildman's essay stands somewhat apart from that chorus of admiration of class struggle and revolutionary energy of the masses. It is a perceptive discussion of the mentalities of army officers.

The weakness of the revisionist interpretation is obvious. At the height of Bolshevik popularity they had only about 50 percent support of the soviets and 25 percent support of the national vote. That was certainly not a national mandate. Support of the masses for soviet power must be seen in the cultural and political context of the time. It did not mean support for socialism or for the Bolshevik party, or for dictatorship or for the disbandment of the Constituent Assembly or for Red Terror. It only meant support for democratic peace, agrarian reform, and improvement in food supply, that is, tasks the Bolsheviks failed to resolve. The class methodology chosen by these authors blinds them to the diversity and unpredictability of politics. The social reality of 1917 was not just proletarians acquiring consciousness, the bourgeoisie, and the party leading the masses. It was also liberation from restraint and rebellion against any state authority. It is wrong to see social groups in 1917 as pursuing their rational objectives. Collapse of state authority unleashed irrational fears, frustrations, and violence. There were also drunken hordes of deserters, devastating the Russian countryside, an explosion in crime, including rapes, assaults, burglaries, massacres of officers, and pogroms (some elements of these processes are repeating themselves now). The authors in these essays do not mention the Red Terror, both Bolshevik-sponsored and spontaneous. They do not even mention that the Mensheviks and SRs won elections to the soviets in spring 1918 and that the Bolsheviks disbanded these soviets (see my book, *The Mensheviks after October* [1987]). These facts are simply ignored because the Bolshevik claim to legitimacy based in soviets would be impossible to defend. Another problem with revisionists is that they take official Soviet documents at face value. David Mandel has no doubt that Kineshma workers voted against the Constituent Assembly. How do we know that so-called worker resolutions were not passed by some party cell claiming to speak in the name of workers, as became standard practice later? The

record of workers' actions in 1918, however, shows that as everywhere else they were increasingly disappointed with the Bolsheviks. Mandel is simply wrong in his claim that workers in that region remained loyal Bolshevik supporters later in the civil war. All the evidence points to the contrary. The revisionists claim to be proponents of social history. Yet for some reason they are selective in the kind of social groups and the time framework on which they choose to focus. Half a dozen books are devoted to workers in the second half of 1917. The volume simply ignores all the data on anti-Bolshevik elections, strikes, mutinies, and rebellions, by workers, peasants, and soldiers and deserters from the Red Army when the Bolsheviks were actually in power. The greatest weakness of this group of essays is that the authors follow the traditional February to October scheme. It tends to portray October 1917 as an end result of the revolutionary process. The Bolsheviks won and there is nothing else to explain. In fact, October 1917 was only the beginning of the tremendous social upheaval that lasted another four years.

The discussion of nationality problems in the empire follows the same basic revisionist format. National identity was not developed, argues Ronald Grigor Suny, in either Lithuania or Ukraine. Class identity was developed. Class solidarity was much more important and workers and peasants in the Baltics, Ukraine, and Caucasus allied themselves with the Bolsheviks for a variety of reasons. Several key points are simply wrong in this presentation. First, the Russianized Ukrainian workers were not Bolshevik supporters in 1919 and 1920. They supported the Menshevik Council of Trade Unions, some greeted A. I. Denikin's White Army as liberators, and some joined anti-Semitic organizations. Second, Suny forgets to mention that Ukrainian peasants waged a bloody and prolonged war against Moscow Communists in 1919, 1920, and 1921. This war was led by peasant leaders clearly conscious of their Ukrainian identity and pursuing as a goal Ukrainian statehood. In Suny's presentation one may forget that the Red Army ever existed. It is all about classes and class identity and economic integration made inevitable by history. Suny simply does not mention the key fact that it was the Red Army and not proletarian identity that reconquered the empire by force, crushing dozens of local insurrections, overthrowing legitimate governments recognized by the Bolsheviks, and subjecting the population to merciless Red Terror.

Part 4 is devoted to Leninism, and here the book begins to be interesting because there is a genuine and serious debate. Neil Harding's main thesis in his discussion of Lenin's political thought in 1917 is that Lenin did not seek to establish a dictatorship of the proletariat but rather a commune state modeled on the Paris Commune. Not socialism but state capitalism was on Lenin's agenda. Popular sovereignty based in the soviets was the Russian version of the commune. Lenin is depicted as a Marxist, almost a

Menshevik, who knows the boundaries of the achievable at the given time. Lenin was not about to destroy private property or capitalism but merely wanted to promote popular sovereignty and create preconditions for a possible eventual socialist transformation. The problem with this argument is that Lenin was not a Menshevik. It is misleading to evaluate Lenin's record on the basis of his theoretical writings only. If he had truly been for state capitalism as the best system suitable for Russia, he would have had to support the provisional government because it tried to create state supervision of the economy. Lenin was after power, not state capitalism.

John Keep's brilliant essay examines Lenin's actions as the head of government in the first few months after the coup. We see Lenin planning disbandment of the Constituent Assembly, shutting down the opposition press, protecting unsavory characters from justice, and issuing dozens of decrees. Keep sees Lenin as a head of a junta with no checks to arbitrary power. What Keep has done is subject Lenin's record as a head of government to a cultural analysis. Lenin issues laws himself, ignoring the legislature and relying on secret police accountable only to himself. Lenin's culture is that of a dictator imitating the autocrat. Even a Marxist would have to agree that this record has nothing to do with either a commune state, a bourgeois republic, or a dictatorship of the proletariat.

Part 5 is devoted to historiography and theory. D. A. Longley raises some interesting and provocative questions on the leadership and the role of Bolsheviks and workers in the February Revolution. Edward Acton discusses the socialist theory and various schools of thought on the nature of the revolutionary process in Russia, praising revolutionary energy of the masses and revisionist scholarship. And Baruch Knei-Paz discusses Russian Marxism as a body of ideas.

This book was produced when the Communist regime in Russia was living out its last year. Celebration of the "October Revolution" by some Western historians is increasingly at odds with new scholarship on the Bolshevik coup d'état in today's Russia. Closing this book one cannot help thinking that reassessments of 1917 and 1918–22 are still in order.

VLADIMIR BROVKN
Harvard University

PETER KENEZ. *Cinema and Soviet Society, 1917–1953*. (Cambridge Studies in the History of Mass Communications.) New York: Cambridge University Press. 1992. Pp. ix, 281. Cloth \$59.95, paper \$18.95.

This is not a conventional history of Soviet film under Lenin and Stalin. Rather than investigating Soviet films for their own sake, Peter Kenez is interested in how cinema contributed to the creation and maintenance of the Soviet system. In essence, this book is an analysis of Soviet film's successes and failures as a

propaganda medium, elaborating on themes Kenez presented in *Birth of the Propaganda State: Soviet Methods of Mass Mobilization, 1917–1929* (1985). He shows that the Bolsheviks had a unique understanding of propaganda as a method of education, not manipulation. Through a variety of media, Soviet leaders attempted to inspire sympathy for their economic and political goals. Film was quickly singled out as a particularly promising method of propaganda because of its popularity and its ability to reach the illiterate lower classes with stirring visual images.

Kenez examines his topic from many different angles, looking at the structure of the film industry, the role of censorship, the contents of many representative films, and the composition of audiences. He argues that because the Bolsheviks placed such high hopes in film's abilities to indoctrinate viewers they set themselves up for disappointment. In the early years they faced overwhelming practical difficulties, including shortages of skilled directors and film. When an original Soviet cinema began to take shape in the 1920s, it met with stiff competition from foreign films. Audiences were more interested in imports from Hollywood than in the products of the golden age of Soviet cinema. The rise of Stalin brought new problems. Foreign competition was summarily ended, but the political demands placed on films mounted, often coming from Stalin himself. Directors faced an ever more invasive censorship apparatus that made films very difficult to complete.

In this study, Soviet directors receive more attention for their political conformity than for their artistic innovations. Never shy with his opinions, Kenez challenges the notion that the directors from the Soviet golden age, like Sergei Eisenstein or Dziga Vertov, were makers of "revolutionary" films in any meaningful sense of the word. Although their movies were stylistically innovative, these directors willingly worked for the interests of the state and assumed that political authorities had the right to interfere in the most minute details of their films. In Kenez's view, Soviet directors were not helpless victims in a system that sometimes devoured them; instead, they were the system's willing accomplices in the creation of a large-scale indoctrination network.

Although Kenez does an excellent job of portraying the political demands placed on Soviet film makers, film viewers get less attention, particularly in the sections on Stalinist film. In his examination of the 1920s, Kenez concludes that films were successful in inverse relation to their propaganda value for the regime. He has a more difficult time explaining the growing number of film viewers in the Stalin era, when films were much more heavily politicized. Writing off the possible appeal of Stalinist comedies, he speculates that movie viewing went up simply because audiences had few other chances for entertainment.

Kenez ends his study in the postwar period, when film production dropped to an all-time low. During these bleak years, the state placed so many political

demands on directors and scenarists that very few films could be completed at all. Instead audiences were offered reruns from the 1930s along with light-hearted entertainment films seized from the Germans during the war. There is considerable irony here. While the Stalinist leadership was campaigning against foreign influences, at least some viewers were enjoying cheerful German musical comedies. Kenez's tone is somber in these last chapters, as he underscores his continual themes of political repression and censorship. It is possible, however, for the reader to draw a more optimistic message from this impressive study. In essence, the Stalinist propaganda apparatus devoured itself, revealing not the power but rather the weakness of the Soviet propaganda state.

LYNN MALLY
University of California,
Irvine

R. CRAIG NATION. *Black Earth, Red Star: A History of Soviet Security Policy, 1917–1991*. Ithaca: Cornell University Press. 1992. Pp. xvi, 341. \$29.95.

The demise of the Soviet Union provides a unique opportunity to review the history of Soviet security policy as a whole. R. Craig Nation takes the story from Lenin to Gorbachev in a thoroughly researched, clearly organized, and eloquently written history that will provide stimulation and pleasure to both specialists and general readers.

Inevitably, the subject is profoundly controversial. One can only endorse the author's aim "to empathize with Soviet decision makers and to see the world as it appeared when viewed from Moscow" (p. xv). The approach is thoughtfully applied, although the treatment of World War II and the origins of the Cold War occasionally crosses the line between empathy and apology, echoing the views of revisionist historians. For example, Nation doubts "the West's willingness to accept the consequences of the Soviet victory and to engage with good spirit in the kind of reciprocal diplomatic interaction for which Yalta provided a prototype" (p. 154). Does he recall the many Soviet actions following Yalta that troubled Roosevelt in his last days and undermined his hopes for postwar cooperation with the Soviets? And Soviet criticism of the West's "failure to establish a second front prior to 1944" (p. 156) is presented without any suggestion that the Soviets' plight derived from Stalin's deal with Hitler, or that logistically the complex maritime invasion of 1944 was simply not feasible earlier.

Nation describes the failure to bring "the victorious USSR into a new structure of world order" as a "lost opportunity" (p. 159). The main responsibility is assigned to the West, particularly to Harry Truman. Stalin's contribution was his "insistence upon an extremely ambitious definition of minimal security requirements" (p. 160). Can one really equate the impact of Truman's rudeness on the sensitive Mo-

lotov with that of Stalin's "ambitious definition" on the fate of one hundred million Europeans? One shares the author's disappointment at "failure cooperatively to resolve the problem of postwar world order" (p. 173), but not his apportionment of blame.

The chapters on the Khrushchev and Brezhnev years achieve a good balance of judgment on the causes of expanding East-West arms competition, especially in nuclear arms, and the costly failure of the Soviet concept of peaceful coexistence, which "could not permanently be combined with a confrontational diplomacy and a two-camps mentality" (p. 284). It was the task of Gorbachev and his colleagues, recognizing that "the foreign policy dogmas of the Brezhnev leadership did not correspond to realities" (p. 284), to find the way out. Discussion of that process, including an excellent analysis of the "new thinking" in foreign and security policy, is offered in the final chapter.

The Gorbachev critique of Brezhnev's security policy led back inevitably to Lenin and Stalin, and Russian specialists are now using an enormously expanded supply of archival and memoir literature to reexamine the first decades of Soviet history. Interestingly, they seem to share Eduard Shevardnadze's view that seventy-four years of Soviet confrontation with the West were, in the final analysis, the result of projecting class struggle to the world stage, and to the relations of nations. It is no accident that when the premise of communist ideology was repudiated, the rest of the edifice followed, and with it the Soviet regime's commitment to the overthrow of capitalism, which dated not to 1945, but to 1917.

HERBERT J. ELLISON
University of Washington

HAIM SHEMESH. *Soviet-Iraqi Relations, 1968–1988: In the Shadow of the Iraq-Iran Conflict*. Boulder, Colo.: Lynne Rienner. 1992. Pp. x, 285. \$42.00.

In this thorough study of Soviet-Iraqi relations, Haim Shemesh traces the ups and downs of a stormy association, characteristic of Moscow's experiences in the Third World. He opens with the historical background from 1944, when diplomatic relations were established, to 1968, the year of the Ba'athi coup. Shemesh then examines the period of initial coolness (1968–71), focusing on the issues that gave rise to the differences between Moscow and Baghdad: the fortunes of the Iraqi Communist Party and of the Kurdish minority, Iraq's relations with Iran, the Arab-Israeli conflict, and regional politics of the Persian Gulf. He next covers the "honeymoon" years (1971–73), marked by concurrent deterioration of Iraqi-Iranian and Soviet-Egyptian relations, and the resulting rapprochement between Baghdad and Moscow, culminating in a Treaty of Friendship and Cooperation. Following this Shemesh details the events between 1973 and 1975, which were characterized by a

gradual cooling of relations and highlighted by the war against the Kurds and the Algiers accord between Iraq and Iran. The author analyzes the impact of the agreement on the Soviet Union and shows how Moscow-Baghdad relations reached their nadir in 1979. He devotes one chapter to the Iran-Iraq war (1980–88), detailing the fluctuations in Soviet-Iraqi relations during this major conflict. In the final section, he summarizes his conclusions.

Shemesh's research in Soviet, Iraqi, and Western sources is impeccable and the factual material is presented succinctly and objectively. Disagreement with his findings, in my opinion, is possible only in the realm of interpretation. The theme of the work is summed up in the following sentence: "The policy of the Ba'ath Iraq toward the Soviet Union was determined first and foremost by its need for Soviet military support vis-à-vis Iran" (p. 249). In this sense, Baghdad's need for Soviet assistance is seen as paralleling that of Egypt and Syria after the 1967 war against Israel (p. 253): the existence of a powerful, Western-backed enemy made it imperative for these Arab states to secure military and political support from the Soviet Union.

This line of reasoning, however, disregards the differences in the internal situations of these Arab countries. Whereas political opposition to the Egyptian and Syrian leaders was relatively weak, the same was not true of Iraq. The Kurds have long fought for the autonomy of their oil-rich region. Because the government had no intention of granting it, the issue had to be resolved by force. It stands to reason that this problem was every bit as important to Baghdad as the question of sovereignty in the Shatt al-Arab. In any event, in order to defeat the Kurds, the Ba'ath had to secure large-scale deliveries of Soviet arms. Moreover, in the early 1970s Iraq was determined to nationalize its oil industry, another important feature that set it apart from Egypt and Syria. Shemesh is correct in arguing that the Soviets could not absorb Iraq's petroleum production in case of the anticipated Western boycott—one reason that he cites to relegate the oil factor to secondary importance in connection with the signing of the 1972 Treaty of Friendship and Cooperation. But, having done so, Moscow lent its official approbation and support for the nationalization, thus virtually guaranteeing Iraq's safety in the face of strong Western displeasure. In short, in contrast with Egypt and Syria, Iraq in the early 1970s was facing momentous domestic problems that I believe overshadowed the importance of Baghdad's rivalry with Tehran. In contrast, in the 1980s, having settled the domestic issues, Iraq's efforts to improve relations with the Kremlin were clearly determined by the exigencies of the war against Iran.

These observations are in no way intended to detract from the exemplary scholarly quality of this book. It is an excellent work by a young scholar and constitutes a welcome addition to the body of litera-

ture devoted to the study of Soviet policies in the Third World.

O. M. SMOLANSKY
Lehigh University

NEAR EAST

MOSHE GIL. *A History of Palestine, 634–1099*. Translated by ETHEL BROIDO. Rev. ed. New York: Cambridge University Press. 1992. Pp. xxvi, 968. \$125.00.

Originally published in three volumes in 1983, Moshe Gil's massive study has aroused both hope and frustration. Hope, because of the extraordinary trove of material that it contains. Frustration, because few students of early Islam (including myself) are competent Hebraists. But now Cambridge University Press has made volume 1 available to a broader audience—for a formidable price, to be sure. Volumes 2 and 3, which contain the 600-plus Geniza documents that are the basis for much of his presentation, remain untranslated. Gil's work will be debated and assiduously mined for a long time to come; what follows can only be a provisional evaluation.

The period studied here is a crucial one. In 634 Palestine was an important if secondary center of Jewish and Christian life. For the Jews of the eastern Mediterranean, the Palestinian Academy was a vital source of religious guidance, and the Byzantine emperors lavished much concern and wealth on the country. By the time the Crusaders arrived in 1099, Palestine was a frontier zone of marginal religious, political, and economic significance. Its governors were mid-rank amirs closely supervised by Cairo, but often there was barely any government at all. The once prestigious Academy had dissolved, the Christian hierarchy played a narrow local role, and the centers of Muslim scholarship were elsewhere: in Cairo, Damascus, even Tyre and Tripoli. This bleak scenario did not emerge full blown with the Arab conquest; the eleventh century was particularly troubled, and Muslim rule saw many periods of security and prosperity. But the overall trend, at least since the fall of the Umayyads, seems unmistakable.

Under such circumstances, it is no surprise that we face source problems. Few documents from early Islamic Palestine have survived, and our literary sources overwhelmingly represent the perspective of writers living in Cairo and Damascus. Gil's achievement thus merits profound admiration; there can be little in any relevant language (Arabic, Judeo-Arabic, Hebrew, Greek, Latin) that he has not scoured. Since usable data come only in bits and pieces, the result is a mosaic, sometimes patchy but overall remarkably complete. Gil might be faulted for his limited use of archaeological materials; one gets a sketchy sense of material culture from his account, and even the monuments and shrines that filled the country are almost invisible. But no one can do everything; what is here is astonishing in range and detail.

Because the sources produced by the three main religious communities of Palestine are highly disparate, Gil constructs a different account of each. In spite of their relatively high numbers, the Christians are the most poorly served; a marginalized and internally fragmented community in a frontier zone, where writing was a Church monopoly, naturally left a poor record of itself. The result (no fault of Gil's) is that in a book with 837 pages of text, only sixty pages are devoted specifically to the Christians, with a marked emphasis on ecclesiastical institutions. The Muslims constituted the ruling stratum and produced a vast body of writing (although little is Palestine-centered); they receive almost 300 pages and are of course a constant presence throughout the book. The character of the Muslim sources means that Gil's account is largely traditional political and military history. He has also assembled invaluable repertoires of Bedouin tribes and Muslim scholars, but he is unable to say much about the structures of everyday life among non-elite Muslims. With the Jews it is quite otherwise. They benefit from Geniza documents relating to Palestine, along with much else; their share is some 350 pages. Here our documentation allows us to enter into the synagogues and homes of the Jewish community. Gil's presentation is diffuse and sometimes hard to follow, and his confrontations with other scholars will be controversial. But his interpretations are important and richly documented.

There are inevitably shortcomings. Partly this is due to the passage of time. The Hebrew original was published a decade ago, and much significant work has appeared since (for example, by Thierry Bianquis, F. M. Donner, Walter Kaegi, Robert Schick, and Irfan Shahid). There are a few infelicities in the translation. Thus, the Lombards are called "Aryans by creed" rather than "Arians" (p. 4; a Freudian slip?). A certain qadi is said to have "prevailed over the Shafi'i *madhhab* in Egypt" (p. 328); presumably he became the head of the Shafi'i's there. One also finds petty errors of fact. For example, the would-be dynast Salih b. Mirdas is called an 'Uqayli, but in fact he stemmed from the Kilab tribe (p. 405). And the name Husam al-Dawla is consistently mistransliterated as Hassam al-Dawla (pp. 726 and following).

These are minor points, but others are less so. Gil takes a literalist view of the numbers given even by legendary or apocalyptic sources: a Byzantine army of 100,000 or even 200,000 at Yarmuk; 500,000 inhabitants in Damascus in 1077; 600,000 deaths from famine in Egypt in 968. Second, he seems too ready to pull socioeconomic evidence out of its chronological matrix: "the facts and findings of the eleventh century can shed light on the entire period, beginning with the conquest" (p. 139). More disturbing is his view of Islam, uniformly portrayed as harsh, rigid, and oppressive. Islamic Palestine was usually a frontier zone; as such it was often afflicted with war and violence, but there was more peace and security than he allows. He stresses the soldiers and Bedouin

among the Muslims, while townspeople and peasants receive little attention.

I do not wish to end on a negative note. The flaws just noted are significant, but they do not undermine the value of this monument of skillful and dedicated scholarship. For scholars in many fields, not just specialists in early Islamic Palestine, Gil's work will long remain fundamental and indispensable.

R. STEPHEN HUMPHREYS
University of California,
Santa Barbara

ANITA SHAPIRA. *Land and Power: The Zionist Resort to Force, 1881–1948*. Translated by WILLIAM TEMPLER. (Studies in Jewish History.) New York: Oxford University Press. 1992. Pp. x, 446. \$59.00.

The Jews, writes Anita Shapira, have traditionally viewed themselves as "a people abhorring violence in any form," yet Israelis see themselves as "not hesitat[ing] to resort to force when deemed necessary" (p. viii). In this book, Shapira sets out to understand how such a reevaluation of values took place among Palestinian Jewry (the *Yishuv*) from the beginning of Zionist immigration to the creation of the state of Israel. The result is a rich and sophisticated work that nicely complements more conventional political-historical studies of the Arab-Israeli conflict.

Shapira offers some analysis of the Zionist Right but concentrates on the views of the *Yishuv's* labor movement, because of its centrality as the dominant political force in the *Yishuv* and its centrism on issues relating to Arabs and the use of force. Beginning with the socialist pioneer immigrants before World War I, Shapira is careful not to read history backward and to assign to the pioneers greater concern or perspicacity about the Palestinian Arabs than they in fact possessed. At the same time, she makes an important point—which shows up in various forms throughout the book—that many Zionists practiced a form of self-censorship that prevented them from publicly acknowledging the existence and legitimacy of a Palestinian Arab national movement.

The Zionist labor movement's chief response to the growth of Palestinian Arab nationalism after World War I was to form what Shapira calls a "defensive ethos" of power. According to this ethos, the Zionists were engaged in a peaceful and socially just developmental enterprise; Arab attacks against Jews were dismissed as pogroms carried out by agitated mobs incited to violence by reactionary effendis opposed to the constructive socialist program of Labor. Tenacity, an evolutionary approach to nation-building, and a commitment to settle the land were the hallmarks of the defensive ethos. The ideal type associated with this ethos was the farmer willing to defend his land; the heroic defense of the border settlement Tel Hai was this ethos' most powerful myth. Events after 1936, however, challenged and transformed the de-

fensive ethos. The Arab rebellion "led to a slow shift in the meaning of the concept *power* from the sense of a 'critical mass' to physical-military power" (p. 219). The Jewish warrior appeared alongside the farmer-fighter as a heroic figure. The new settlement model was not Tel Hai but Hanita, an isolated outpost founded and defended by paramilitary forces. At the end of World War II, the leaders of Mapai, the dominant labor party, displayed an increased commitment to military activism. This offensive ethos was embodied in the Palmach, the *Yishuv's* first permanently mobilized military force, which led the Zionists' struggle for statehood.

The book's later chapters center around generational differences between the Zionist immigrant "fathers" and their Palestinian-born "sons," differences that, Shapira argues, played an essential role in the transition from a defensive to an offensive ethos. The analyses here of the United Kibbutz movement, the labor youth movements, and the fighters of the "Palmach generation" demonstrate Shapira's consummate skill as a collective biographer of the builders of *Eretz Israel ha-ovedet* (the Labor Zionist Commonwealth). Here, as throughout the book, Shapira sifts through a vast body of material, ranging from essays, poems, and memoir literature to the unpublished minutes of political party and youth group meetings. Shapira interprets these sources with sensitivity and insight.

Shapira writes with power, compassion, and warmth. The translation of her Hebrew manuscript does not always do her prose justice; the early chapters feature several awkward constructions and infelicitous renderings. This quibble aside, Shapira has written a landmark book that is an outstanding contribution to the history of Zionist political thought and culture.

DEREK PENSLAR
Indiana University,
Bloomington

MICHAEL M. LASKIER. *The Jews of Egypt, 1920–1970: In the Midst of Zionism, Anti-Semitism, and the Middle East Conflict*. New York: New York University Press. 1992. Pp. xiv, 326.

Michael M. Laskier's new book is the most recent of several studies that have appeared over the past twenty-five years devoted to Egyptian Jewry during the modern period. It is an important complement to Jacob Landau's pioneering work, *Jews in Nineteenth-Century Egypt* (1969), to Gudrun Krämer's solid monograph, *The Jews in Modern Egypt, 1914–1952* (1989), and to the volume of diverse and more specialized studies edited by Shimon Shamir, *The Jews of Egypt: A Mediterranean Society in Modern Times* (1987). Of these earlier books, the one that Laskier's study comes closest to in the time period covered is Krämer's. The focus, however, is quite different.

Laskier wisely avoids retracing the socioeconomic ground that Krämer admirably covered.

As the subtitle implies, Laskier's book focuses on Zionism, anti-Semitism, and the Middle East conflict and their impact on Egyptian Jewry from the end of World War I through the eventual liquidation of the community. Laskier makes extensive use for the first time of Israeli government archival material. (Krämer made good use of the Zionist Archives in Jerusalem, but not state and military archives. Unlike Laskier, however, she also had access, albeit limited, to Egyptian archives.)

Laskier argues that despite the dampening of the early enthusiasm for Zionism during the 1920s and 1930s, the movement achieved some success due to the pro-Zionist Egyptian Jewish press and a small but dedicated elite of activists and organizations. During the 1940s, the movement gained momentum, as it did everywhere in the Jewish world. Laskier rightly argues—as I did in *The Jews of Arab Lands in Modern Times* (1991)—that the decline of Egyptian Jewry after World War II, although hastened by the Palestine conflict and the ensuing Middle East wars, was part of the wider phenomenon of the decline of the non-Muslim minorities and particularly of foreign ethnic minorities as pan-Arab and pan-Islamic nationalism waxed and European domination waned. He does not, in my opinion, give enough weight to the effects of anti-Semitism adopted from the West among such movements as Young Egypt and the Muslim Brotherhood during the 1930s, seeing it only as significant under Gamal Abdel Nasser after the Suez Campaign of 1956. Laskier is at his best in detailing the activities in Egypt of agents from the Jewish community in Palestine during the 1940s and 1950s, the emigration process, and the underground apparatus of the Mossad le-Aliya, the clandestine organization in charge of bringing Jews to Israel. He also sheds some further light on the espionage debacle of 1954, officially dubbed Operation Susannah, but most commonly known in retrospect as “The Mishap.” This incident involving Egyptian Jews recruited to carry out sabotage operations was disastrous not only for its further undermining of the already weakened position of the Egyptian Jewry but also for its effect of increasing the alienation between Israel and the Egyptian revolutionary regime. Laskier brands the affair, which was apparently planned by officers in the Israeli Ministry of Defense without government knowledge, “not merely an error of judgment but also an irresponsible act politically” (p. 246).

The strength of this book is that it covers new ground in several important areas not fully dealt with by Krämer, and in particular it examines the period after the fall of the monarchy when the actual dissolution of Egyptian Jewry took place. Its weakness is a certain disjointedness and a tendency to become bogged down in minor details that distract from the coherence of the broader picture. Like Krämer, Lask-

ier takes no interest in the cultural, intellectual, or spiritual life of Egyptian Jewry. Those important aspects await historical treatment.

NORMAN A. STILLMAN

State University of New York,
Binghamton

AFRICA

WILMOT G. JAMES. *Our Precious Metal: African Labour in South Africa's Gold Industry, 1970–1990*. Bloomington: Indiana University Press or David Philip, Cape Town, or James Currey, London. 1992. Pp. ix, 188. \$25.00.

We do not lack accounts of labor repressed and coerced by the machinations of capital and the manipulations of the apartheid state and which advocate the overthrow of the existing South African order. Wilmot C. James's work, although informed by such accounts, finds their explanations and predictions suspect. States have their own agendas. A black-controlled government might or might not nationalize the mines and likely not transform social relations. The African National Congress may advance black power and the National Union of Miners (NUM) may give preferential treatment to black goldminers. But neither will fundamentally alter class relations nor eradicate racism. How has the author arrived at this conclusion?

This is a work of historical sociology “where original source material and historical methods of data collection are used to answer sociologically motivated questions” (p. 15). Much useful information was also obtained from “a number of individuals in the corporate and trade union worlds” (p. viii) as well as in government. But others, as the preface notes, denied the author interviews or assistance because of his color. He recognizes debts to other scholars, especially to Jonathan Crush, Alan Jeeves, and David Yudelman's *South Africa's Labor Empire: A History of Black Migrancy to the Gold Mines* (1991). Their work and his overlap in terms of evidence used, issues addressed, and emphasis given to continuities. But James concentrates on the very recent past and relations between the NUM, formed in 1982, and the Chamber of Mines. These events include the working conditions under which NUM emerged, adjustments to the color bar before its legislated demise in 1987, and the battle in the single-sex compounds between the union and management for hearts and minds. Legal evidence generated by the miners' strike of 1987 illuminate relations in the compounds of three mines, but no formal interviewing techniques are used.

The NUM made headway, but management bested it in the 1987 strike with mass dismissals. The numbers of migrants from outside the republic dwindled but labor stabilization, an NUM objective, has not occurred. De facto job reservation persists.

James stresses differences among the mining houses that control the mines and form the Chamber of Mines. He finds them seldom in lock step and Chamber policy neither coherent nor premeditated. The Anglo-American Corporation has sometimes followed its own agenda and, as the largest employer of black labor in the mines, oscillated between permissive and repressive actions in responding to organized black labor. Whether dealing with black labor, white labor, or the state, capital, even when divided, has worked for negotiated settlements. James also finds that by choice or necessity the other players have also opted for negotiation. This historical experience will, he concludes, condition the pattern of change in the future.

The various groups contending for wealth, power, and status in contemporary South Africa, especially those involved in its industrial development, are extremely well set out in this book. Whether their current and future relations are adequately explained is problematic, but the work is sufficiently well grounded in relevant evidence and current research to be plausible.

ROBERT KUBICEK
University of British Columbia

ASIA

DENIS TWITCHETT. *The Writing of Official History under the T'ang*. (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press. 1992. Pp. xi, 290. \$49.95.

An authority on T'ang history, Denis Twitchett examines the nature of official historical writing in traditional China generally, and the writing of various historical works as a bureaucratic and political activity in T'ang times (618–907) particularly. The first part of this critical study deals with the institutions that gathered and compiled historical materials. In the second part Twitchett describes the official compilation of historical works such as histories of institutions, encyclopedias, and collections of documents. The third part examines the writing of the national history, which was based on the court diaries (*ch'i-chu chu*), the records of administrative affairs (*shih-cheng chi*), the daily calendar (*jih-li*), the biographies, and the veritable records (*shih-lu*), among other sources. The compilation of the veritable records and the national history (*kuo shih*) in government offices was a T'ang innovation, and the practice was continued in later dynasties.

Using all available primary data and secondary studies, the author shows impeccable scholarship in describing every official historical work, whether it is extant or not. He demonstrates a rich and wide knowledge of T'ang history and its sources by looking into each stage in the official enterprise of historical writing, informing the reader of every detail in the successes and failures of this enterprise. The modern

historian might be frustrated by the method of compilation that emphasizes the central administration. But Twitchett points out that the exclusion or disregard of local events is understandable because the basic aim of official historiography was the writing of a record of the exercise of dynastic power (p. 199).

In the writing of the national history, rebellions and political events interrupted and interfered with the process of compilation and writing. It is interesting to learn about some of the historians who made important contributions under difficult circumstances. Basing their work on the T'ang national history, which was by no means complete, historians in the tenth century rather hastily compiled the dynastic history of the T'ang (*Chiu T'ang Shu*), adding available information at the time. As a consequence, the *Chiu T'ang Shu* that survives today is not a balanced account. Due to the uneven distribution of facts across long periods of time, Twitchett warns the students of T'ang history that they should be extremely cautious in using the materials for statistical analysis and comparison covering the entire dynasty (p. 205).

Twitchett concludes his study by carefully examining the Basic Annals and the Monographs of the *Chiu T'ang Shu*, but he states that it is still premature for him to scrutinize the biographical section. One nevertheless awaits his survey of that important part, for no one is more qualified to explore the merits and shortcomings of T'ang biographical writing.

Any serious student of T'ang history should read this book, for it provides vital information and important criticisms of T'ang historical sources. It also sheds new light on the nature of official historiography in traditional China.

JING-SHEN TAO
University of Arizona

THOMAS KEIRSTEAD. *The Geography of Power in Medieval Japan*. Princeton: Princeton University Press. 1992. Pp. 181. \$27.95.

In this book, Thomas Keirstead offers a new portrait of the landscape of the Japanese *shōen*, or estate, the central economic institution of the era. The author applies the ideas of Michel Foucault and his followers to illuminate medieval Japanese social history as skillfully as others have done for Tokugawa Japan.

In the initial chapter, "In Go-Sanjō's Archive," Keirstead argues that Emperor Go-Sanjō's *shōen* prohibition decree of 1069 "established a new discourse for landholding," providing the first condition for the possibility for the estate system "as system" (p. 19). He recognizes a break in the twelfth century in which "mature" *shōen* of the commendation type became "administratively and juridically distinct from the public domain" (p. 19). For Keirstead, the proliferation of commendation-type estates in medieval Japan represents a "fundamental reordering of space and

society . . . that encouraged and underwrote the production of the estate form and legitimized its possibility" (p. 17). The next three chapters explore aspects of the alliances between absentee estate proprietors and subject peasants made possible by the act of commendation.

In chapter 2 ("The Rhetoric of Identity"), for example, Keirstead examines the *hyakushō* (*shōen* residents), the subjects on whom estate proprietorship was based. Chapter 3 shifts the focus to maps, surveys, and rent rolls—the "official transcripts" of the *shōen*—that "mapped (estates) onto a new, characteristically ordered, space" (p. 46). In chapter 4 the author analyzes the "rituals of rebellion" as expressed in documents of protest, documents he argues represent a "process of mutual constitution" within a "context of discord" (p. 97). Chapter 5 concludes by considering the debate over the decline of the *shōen* system.

Keirstead rejects the view of the *shōen* as a fixed economic system of owner exploitation of peasants, instead interpreting it as a cultural form, a continuously evolving "field of struggle" (p. 108) within which both owners and residents constantly redefine their relationship. It is in this conflict between owners and *hyakushō* that the author finds the meaning of the *shōen* system.

In discussing *shōen* decline, Keirstead argues persuasively that, as a continuously renegotiated field of struggle, the *shōen* itself produced new discourses about power and landholding that helped "refashion the realm" so that there was no longer a place for the *shōen* (p. 115). By the mid-fifteenth century, he says, the "estate system was being dislodged by language that substituted the village for the estate, 'public good' for the estate proprietor, and a generic peasant-subject for the *hyakushō*" (p. 116).

There is a wealth of information and keen insight in each chapter of this slim work. Within only 118 pages of text, however, the author is sometimes wordy and repetitious, introducing perhaps more explanatory material than is necessary. Moreover, many readers will surely struggle with the jargon of the Foucauldian approach. But these minor quibbles aside, Keirstead has written a stimulating work that will force us to reconsider the medieval estate system in new and different ways, if not with a different vocabulary.

G. CAMERON HURST III
University of Kansas

KARL F. FRIDAY. *Hired Swords: The Rise of Private Warrior Power in Early Japan*. Stanford: Stanford University Press. 1992. Pp. ix, 265. \$32.50.

As portrayed in virtually all survey textbooks, one of the pivotal events of traditional Japanese history was the shift from rule by the imperial court to rule by the warrior class. The founding of a military government

by Minamoto Yoritomo during the 1180s in Kamakura marked the beginning of a new era in Japan's history. Most surveys explain that this domination of Japan's government by the military class became possible due to the inability of the civil government in Kyoto to maintain control of the provinces and was abetted by its lack of interest in governmental and military affairs. The imperial government generally comes off as ineffectual, with the court nobles more interested in the literary arts than in the more serious affairs of state. In brief, according to general texts, the imperial government lost its power because of inattention to military affairs in the provinces.

Karl F. Friday challenges specialists in the field of Japanese history as well as teachers of survey courses to lay these generalizations to rest for good. He sets forth an elegant argument showing that, in fact, the imperial government paid attention to the provinces and especially to military affairs. He clearly demonstrates that the alterations the Kyoto government made to the military structures established by law in the eighth century did not represent an abandonment of interest in military affairs. Rather, the changes were a rational, calculated response to changed conditions and were specifically designed to meet the new needs for military force perceived by the central government. Through the careful use of primary documents, Friday shows that the Kyoto government was indeed attending to business. It deliberately initiated changes from a peasant conscript infantry described in the law codes to a more highly skilled private army of mounted warriors armed with bows and arrows.

With his detailed discussions of the structures of the eighth-century peasant army, the variety of local constabularies, and the emergence of the warrior class from local gentry roots, Friday's book may at first seem daunting to the nonspecialist. The arguments and discussion of the evidence are lucid and clearly drawn from primary documents, so that with only a little effort in sorting out the terminology, the main points of the book are readily accessible. This look at the development of the warrior class in early medieval Japan casts a whole new light on the workings of the Heian government. Far from being a bunch of dilettantes, the Heian elite now appear as serious governors adept at altering the institutions created during the eighth century reforms to meet the changing demands of later times. In many ways the central government's use of private military forces to serve its needs for keeping order in the capital and in the provinces paralleled the development of private landholdings to meet the elite's needs for a stable economic base.

Friday raises issues that all teachers and scholars must take into account. He argues that the Heian elite took the job of governing seriously and did it well and that the establishment of the warrior class as the ruling class occurred more tentatively and gradually than earlier scholarship indicated. This book should

also cause scholars to look anew at the Heian courtiers who appear equally adept in the arts of governing and literature.

ELIZABETH S. SATO
Raymond Walters College
University of Cincinnati

KYOKO HIRANO. *Mr. Smith Goes to Tokyo: Japanese Cinema under the American Occupation, 1945–1952*. (Smithsonian Studies in the History of Film and Television.) Washington, D.C.: Smithsonian Institution. 1992. Pp. xvii, 365. \$34.95.

This exhaustively researched and clearly written study surpasses all previous scholarship on American cinema censorship in occupied Japan from 1945 to 1952. Kyoko Hirano has harvested declassified government documents, censored screenplays, and dozens of interviews with former occupation officials and Japanese movie directors to produce this even handed account of the inherent contradiction of post-surrender film policy: "the Allies had come to Japan to bring democracy; however, in order to disseminate and inculcate democratic ideas, they had to suppress some of the ideas that might have inhibited the growth of the new ideology" (p. 45).

The Americans quickly banned 236 Japanese movies made between 1931 and 1945 as militaristic or "feudalistic" and imposed both civilian and military censorship on all films shown in Japan, both foreign works and the nearly 1,000 new Japanese productions appearing during the occupation. Until 1949, scripts were translated into English for prior approval; completed movies had to be cleared until 1952. Japanese directors were forbidden to depict nationalistic themes (even Mt. Fuji) and were told to encourage democracy instead. The Americans were particularly vigilant about insisting that Emperor Hirohito not be portrayed as a war criminal.

Hirano notes the irony that "the same filmmakers who had promoted the war effort up until August 1945 immediately began to make films promoting democracy" under the Americans (p. 147). Later in the occupation the censors encouraged fewer such "idea pictures" and suppressed only a handful of forbidden subjects, leaving the Japanese studios freer to produce movies for entertainment and profit. Hirano credits Japanese film companies and directors with great skill in standing up to the Americans' most extreme demands. Nonetheless, U.S. troops and tanks were mobilized to help remove striking unionists from the Tōhō lots in August 1948, a sign of American concern over the ideological potential of movies once communism began posing a threat elsewhere in Asia.

Hirano demonstrates that the Americans systematically used censorship to help democratize Japan, restore it to membership in the world community, and enlist it "as an ally in defending American inter-

ests" in the Cold War (p. 261). Perhaps because she believes that "occupation period films and wartime films alike were substantially apolitical" (p. 10), Hirano seems reluctant to dig too deeply into issues of artistic freedom amid ideological manipulation by the United States. Only occasionally does she address the cultural as well as political hegemony of the victors (for example, a comic description of censors who encouraged baseball as a "democratic," "good old sport" [p. 176]). Hirano's account sometimes reads like a chronology, and it proffers a highly conventional interpretation of overall occupation policy. But its accumulation of evidence and diverting forays into the Japanese film world will convince most readers that the Americans equated Japan's post-surrender interests largely with their own and shamelessly used censorship to promote an American vision of democratic ideals.

THOMAS R. H. HAVENS
University of California,
Berkeley

JACKSON H. BAILEY. *Ordinary People, Extraordinary Lives: Political and Economic Change in a Tōhoku Village*. Honolulu: University of Hawaii Press. 1991. Pp. xii, 259. \$30.00.

This study of rural change in northern Japan chronicles the transformation of an isolated, impoverished, "dismal" fishing village into a community at the "forefront" of economic experiments in regional development. Jackson H. Bailey, who has been intimately involved with the people and community of Tanohata (Iwate prefecture) since 1972, draws on interviews as well as written sources (government reports, local histories, family records, and newspapers) to chart village efforts over the past several decades to enter the "mainstream of Japanese life" (p. 8).

The book highlights the role of the "dynamic, even charismatic" elected mayor, who, over five consecutive terms in office, "presided over twenty years of phenomenal social and economic change" (p. 12). Mayor Hayano focused on improving the local infrastructure, specifically education, transportation, and health facilities. To build group cohesiveness as well as highways, under his leadership Tanohata officially designated a village bird, flower, and tree; wrote a village charter, which is recited in school; and in numerous other ways used "publicity and symbolic gestures to gain attention and recognition for Tanohata in the outside world and to raise the sights and deepen the sense of identity of the village people themselves" (p. 80). By promoting a tourist hotel, inviting Americans to teach English, and encouraging teachers to travel abroad, the mayor also opened up the village to outside influences. These reforms eventually stimulated new jobs and industries.

Over 80 percent of the capital for development

came from outside the village, mainly from prefectural and national government offices. Fortunately, the early years of Tanohata's push to develop—the 1960s—coincided with a boom time in Japan that made financial sources available to the village. Unfortunately, the book does not fully explain how or why Tanohata's energetic mayor, whose education stopped after the seventh grade, managed to pull in more prefectural revenue than any other village in the prefecture or why this man, whose entire life was spent in Tanohata, also successfully competed for funds from the central government, dominated by Liberal Democratic Party politicians and well-heeled career bureaucrats.

It would have been interesting to interview the mayor about his impressive contacts. We read that he and his associates "proved adept at establishing connections with appropriate prefectural and national offices" (p. 127). How exactly did Hayano gain the friendship and support of key members of the Diet who, for example, helped "bring investment for basic fishing port facilities and safe-haven harbor construction" (p. 153) to the community? The answer to this question is crucial for, as Bailey writes, "In the world of Japanese politics, connections mean even more than in other democratic societies" (p. 189).

This case study, while providing a welcome addition to regional history, invites even closer inspection of the important topic of local leadership and its articulation with the prefectural and central government throughout Japan's modern history.

GAIL LEE BERNSTEIN
University of Arizona

SARAH F. D. ANSARI. *Sufi Saints and State Power: The pirs of Sind, 1843–1947*. (South Asian Studies, number 50.) New York: Cambridge University Press. 1992. Pp. xix, 178. \$54.95.

As a landholding and spiritual elite, the *pirs* of Sind stood between the Muslim people and the rulers of the lower Indus River region. Sarah F. D. Ansari examines the complex relationships between this elite and the British government of India. She draws extensively on British official and unofficial papers, supplemented by interviews with many *pirs* in contemporary Pakistan, newspaper articles, manuscripts, and published works in Urdu, Sindhi, Persian, and English.

Sind, brought under Muslim rule by the Abbasid Khalifat (from 711), developed a distinctive class of hereditary Sufi saints and keepers of the tombs of their saintly ancestors. These *pirs* received extensive land grants and revenue from precolonial rulers as well as the intense spiritual devotion of communities and individuals throughout Sind. The British colonial state sought to harness the influence of this class. Although Ansari briefly surveys the origins and relationships of this class with precolonial rulers, she

concentrates on crises of state power where its members played key roles in either supporting or challenging British colonial rule.

Ansari argues that the degree of collaboration by the *pirs* determined the capacity of the government to rule Sind. Sind's "premier religious family," that of "the Pir Pagaro" (p. 58), proved central in two of the crises of colonial rule Ansari examines. Prominent among the devotees of the Pir Pagaro was the Hur community (focused on the "Hur Union," which was classed by the British as a "criminal tribe") (p. 75). In the 1890s many Hurs "rebelled" against the British. The British proved unable to restore order without the support of the Pir Pagaro; he gave that support only when the British threatened to withdraw his personal privileges. During the late 1930s and early 1940s, in contrast, the incumbent Pir Pagaro himself mobilized Hurs against British rule. The government subdued this challenge only through massive military force and the execution of the Pir.

The other crises she examines also demonstrate both how the government relied on the *pirs* for control over the people of Sind and, conversely, how most *pirs* depended on the government for the preservation of their class interests. The Khilafat Movement (1919–24) proved that most *pirs* "were not equipped intellectually or emotionally to live with the consequences of their support for pan-Islamic agitation" (p. 95) when its anti-British aspects threatened "the privileges conferred on them and preserved by British rule" (p. 100). As Pakistani independence approached, the *pirs* "adopted a very flexible attitude, and invested their support where they felt it would bring them most return" (p. 128), eventually associating themselves with the Muslim League. A brief epilogue continues to 1990 Ansari's argument that the *pirs* were pragmatic, self-interested power-brokers.

Ansari's well-researched doctoral thesis has emerged as a tightly focused monograph. Future scholarship can extend our understanding through studies of the equally significant non-*pir* elites (such as the *mirs* in the countryside and commercial groups in the cities) and especially of the non-elites of Sind. Overall, scholars will find much in Ansari's work on Sufi saints and state power in Sind to incorporate into larger theoretical frameworks about imperialism or the history of South Asia.

MICHAEL H. FISHER
Oberlin College

DAVID NEAL. *The Rule of Law in a Penal Colony: Law and Power in Early New South Wales*. (Studies in Australian History.) New York: Cambridge University Press. 1991. Pp. xiv, 265. \$59.50.

From the first page, David Neal engages the reader's sympathies with the tale of Susannah Holmes and Henry Kable. After forming a liaison at Norwich

Castle Jail in 1786, the pair were separated, dramatically reunited, married, deported with the first convict ships to New South Wales, and destined to initiate the first civil case held in Australia's history. Throughout the book the Kables periodically appear, serving as symbols of the British colony's changing fortunes.

My main criticism of Neal's book is that there are not more anecdotes, more personalities like Holmes and Kable, to add human interest and texture. But then Neal's study is in a quite different mold than Robert Hughes's best-selling convict epic *The Fatal Shore* (1987). His intention, Neal tells us, is not to write a history of convicts but of ideas. Rather than evocative case studies, Neal relies on relentless logic.

When did New South Wales make the transition from a penal colony to a free society? If this question has some of the quality of a *Zen koen*, Neal answers it, at one stage, with disarming precision: 1842, the year the colony was granted a partially elected legislature. It is the process of transition, however, not the end result, which mainly occupies Neal and makes this book a significant contribution to Australian historiography.

Neal argues that New South Wales became a free society little by little, and that ideas about the rule of law played a central role in this transformation. One might expect New South Wales' convict population to have scant regard for a legal system that wrenched them from their homeland, and for British authorities to be unperturbed by the claims of a penal colony. This is not the case on either count, Neal claims. Early colonists, including convicts, brought with them a heritage of ideas and rhetoric about the rights of freeborn Britons, Magna Carta, and Blackstone's *Commentaries*. From the beginning, convicts were assumed to possess certain "rights," and colonists pressed for change using the rule of law ideology.

In chapter 2, the book's strongest chapter, Neal gives an illuminating comparison of convictism and slavery, while taking some well-aimed shots at John Hirst's influential book *Convict Society and Its Enemies* (1983). In subsequent chapters he investigates the courts, the magistracy, the police, and trial by jury. Throughout, Neal emphasizes the way in which colonial politics assumed a legal form and the way the legal system determined issues of status and power.

Despite a certain amount of repetition, this is a generally well-crafted work. Neal, a lawyer and a historian, wears both hats admirably. In my opinion, this book earns Neal a place alongside H. V. Evatt, C. H. Currey, and A. C. Castles as one of Australia's leading legal historians.

MICHAEL STURMA
Murdoch University

DAVID DAY. *Reluctant Nation: Australia and the Allied Defeat of Japan 1942-45*. New York: Oxford University Press. 1992. Pp. x, 366. \$45.00.

In this, his third, book, David Day continues to establish his reputation as a historian of the Pacific and especially Australian dimensions of World War II. His focus is the Allied politics and diplomacy of waging war and of steps toward a postwar order, rather than military activities.

Day's thesis is that while the exigencies of war engendered nationally inspired Australian outlooks and initiatives, Australia's independence of mind remained incomplete. On the one hand, Australia conceded much of its wartime military role to U.S. directives. On the other hand, while envisioning a significant role for itself in postwar affairs, it imagined this as part of wider imperial cooperation. Day's assessment is that Australian Prime Minister John Curtin "failed to recognize that a weaker Britain would inevitably mean weaker Imperial bonds; he believed that the Dominions would simply share the leadership. For her part, Britain failed to recognize that the genie of Imperial disintegration could not be put back in the bottle" (pp. 167-68). However well Day turns the argument, it nevertheless remains that, as a small, isolated nation just rising above semi-sovereign status, Australia's realistic options for maneuver were at the time limited. Day also allows himself a conclusion that, half a century later, "the actors have been replaced but the script remains the same, as Australia continues to cling to the sense of dependence upon great power 'protectors,' shrugging off those who point to a possible independent destiny" (p. 311). That, simply put, is an arguable point.

Possibly the most arresting leitmotif in this book is Day's characterization of Britain's, and particularly Churchill's, approach to Pacific war issues. In a number of contexts, on relations with Australia or otherwise, Day alludes to Churchill's perfidiousness. Churchill emerges as a strategist of dubious quality who connived to hoard resources (including Australian) for the fight against the European Axis, dealt with Australia as a colony rather than as a valued ally, and had no compunction about trying to damage U.S.-Australian relations as he grew fearful of an eclipse of returned British influence in the postwar Pacific. Although persisting in his vision of an Allied invasion of Sumatra as a springboard to reconquer Singapore and therefore to restore British prestige, Churchill is also pictured as making British contributions to an assault against Japan sufficient only to enable Britain to claim a major regional voice. Day writes that although Churchill would not make terms with Hitler, he never excluded this possibility with Japan, feeling that his own version of regional strategy was best suited in the service of long-term British interests.

Although he is not a "revisionist" historian, Day adds to and ornaments the public record through assiduous scholarship and pulls few interpretive punches. What he offers is plausible and not infrequently provocative. Whether Day leans too far in

drawing some of his conclusions is for specialists to consider. His book is, however, required reading for them, and recommended reading for others.

HENRY S. ALBINSKI
Pennsylvania State University

UNITED STATES

CAROLYN JOHNSTON. *Sexual Power: Feminism and the Family in America*. Tuscaloosa: University of Alabama Press. 1992. Pp. xvii, 413. \$36.95.

Carolyn Johnston's overview of the history of feminism focuses on women's complex struggles within and against their families. While she fairly presents a wide range of women's history scholarship, Johnston argues a distinctive and controversial position: that the dynamics that have altered and expanded women's position over the last century and a half lie within the family, not without. In particular, Johnston dissents from the notion that women's entry into the labor force has been the driving force in the history of feminism. Wage labor, she insists, is only "redemptive" for a tiny sector of privileged women; the real combination of power and oppression that has fueled the history of feminism can be found within the family.

Johnston is to be commended for developing this family-based history of feminism in an expansive and complex way. Although the ultimate aim of the book is to use history to argue for a "profamily" feminism, Johnston's account shares none of the tendentiousness and feminist-bashing of Sylvia Hewlett's similarly motivated effort (*A Lesser Life: The Myth of Women's Liberation in America* [1986]). Family life, Johnston argues, has been the site of both power and limitation for women in explosive combination. To make her case, Johnston traces the wide variety of family-focused issues within the history of feminism. In her account of nineteenth-century feminism, for instance, she juxtaposes the mainstream approaches that emphasized women's sexual purity and maternal essence with radical advocates of liberalized divorce and critics of state-enforced, male dominance within the family. If anything, her sympathies lie with the latter. The author also argues for a reconsideration of the alterations in women's role during World War II mobilization, which she believes have been too summarily dismissed by historians for their superficiality and subsequent reversal.

Ultimately, however, Johnston is less concerned with the range of feminist belief and program, or with the history of feminism as politics, than she is with how various women lived their principles, a concern that she calls "personal feminism." From this perspective, Johnston finds that relatively conservative, "profamily" types like Antoinette Brown Blackwell and radical, anticoverture activists like Elizabeth Cady Stanton had in common the pursuit of egalitarian marriages and reformed families. For all the possibilities of this focus and her expansive sense of

"family life," however, Johnston's approach marginalizes those nineteenth-century women, for example Susan B. Anthony, whose daily lives were not organized around conflicts over marital power or maternal sacrifice.

Despite the author's best intentions, this approach also marginalizes African-American women, because the public/private distinction, which Johnston relies on to delineate a separate "family" sphere, for most of American history has characterized the experiences of the middle class much more than other sectors. Johnston includes material on women of color, but inevitably as an appendage to the major argument. Thus, it is somewhat disconcerting to find, in the book's final section, that the experience of contemporary African-American women is invoked as a warning to modern feminists not to take the careerist path and thereby abandon the welfare of family and community in the pursuit of individual achievement. The book's argument seems to take a radical turn somewhere in the mid-twentieth century: having envisioned most of women's history as a struggle to enhance women's family-based power and to combat women's family-based oppression, Johnston seems to be arguing that the proper future direction for women should be to reconcile family life and individual identity. Yet the means for doing so, for achieving independence and autonomy, lie precisely in lifelong wage earning historically pioneered by African-American women and otherwise largely outside of the history of American feminism as Johnston has reenvisioned it.

ELLEN DUBOIS
*University of California,
 Los Angeles*

JOE S. SANDO. *Pueblo Nations: Eight Centuries of Pueblo Indian History*. Foreword by REGIS PECOS. Santa Fe, N.Mex.: Clear Light. 1992. Pp. xiv, 282. Cloth \$22.95, paper \$14.95.

The traditional framework of Pueblo Indian life remains. Joe S. Sando of Jemez Pueblo concludes, however, that "judging from this writer's traditional upbringing, it is rapidly disintegrating in the face of a new order and a new lifestyle" (p. 160). In this study, Sando provides a thoughtful overview of the nineteen villages of New Mexico. Readers familiar with Sando's *The Pueblo Indians* (1976) will recognize that his new volume is based on that earlier study. There are, nonetheless, sufficient changes and additions to the first book to justify reading this new book.

Seven of the eight chapters appear here in somewhat revised form, along with a new chapter on the Columbian quincentennial. Sando has also updated and expanded his discussion of "Elements of Change," a chapter that focuses in part on evolving issues in education and economy. Current population estimates and landholding totals for the different

Pueblo communities, several photographs by Marcia Keegan (including the cover illustration of the Corn Dance at Santa Clara Pueblo), and a historical listing of the officers of the All Pueblo Indian Council are welcome aspects of the book.

Sando devotes only eight pages to the quincentennial and perhaps those weary of the anniversary will applaud his restraint. In any event, he makes several intriguing observations. Although he argues there had been previous contact between peoples of the two continents, European exploration and colonization "meant the end of life as the indigenous people had known it" (p. 165). The Pueblo revolt of 1680 naturally altered the Spanish colonial approach in the Southwest, permitting the villages to keep much of their religion and language. The arrival and presence of the Spaniards did affect Pueblo food, cooking, language, government, clothing, and religion.

Cultural conservatism and relative isolation allowed for considerable continuity from one generation to the next. Now in his seventies, Sando is not entirely optimistic about what the future may bring. He offers a candid assessment of critical problems facing today's villages. Head Start and television encourage the use of English as a first language. Lack of knowledge of Native languages would hamper young people from eventually taking leadership roles in ceremonies, where only those languages may be uttered in the kivas. Law enforcement, environmental quality, and limited social services also represent pressing dilemmas. Population growth poses especially difficult questions about land use.

There are topics Sando chooses not to address here and therefore the book ultimately is less of a departure from *The Pueblo Indians* than one might have hoped. This remains, however, an important and rare book. Clearly written and fairly presented, the volume is one of the few histories we have by a Native historian willing to articulate a first-hand synthesis of American Indian history and culture. Academic historians may bemoan the lack of footnotes, the limited bibliography, and other scholarly trappings. They should be willing to trust Sando's knowledge and the integrity mirrored in these pages. Sando has made a valuable contribution to American Indian history.

PETER IVERSON
Arizona State University

T. O. MADDEN, JR. *We Were Always Free: The Maddens of Culpeper County, Virginia; A 200-Year Family History*. Assisted by ANN L. MILLER. Foreword by NELL IRVIN PAINTER. New York: W. W. Norton. 1992. Pp. xxx, 218. \$19.95.

This unusual book combines family history, personal memoir, and a collection of documents. The author, T. O. Madden, Jr., is a retired farmer in Culpeper County, Virginia, who will celebrate his ninetieth

birthday in 1993. Almost fifty years ago he discovered in his father's attic a hidebound trunk that contained family records dating back to the birth in 1758 of his great-great grandmother, Sarah Madden, mulatto bastard child of a black man (name and status unknown) and Mary Madden, a white pauper. From the indentures, bills of sale, tax receipts, account books, and other items in the trunk (reprinted in this book), the author (with the assistance of Ann L. Miller) chronicles the saga of Sarah Madden and her descendants.

Before she was two years old, Sarah Madden was taken from her mother and bound out to a merchant in Fredericksburg who, in payment of a debt in 1767, transferred Sarah's indenture to James Madison, Sr., father of the then sixteen-year-old future president. Serving out her indenture first at Montpelier, then with Madison's son Francis, Sarah Madden became fully free in 1789 and soon moved to Culpeper County where she supported herself and her large family with her skilled needlework. Before she was released from her indenture, she gave birth to seven children; six more children arrived after 1789. No record survives of the paternity of any of these children. Although the author repeatedly emphasizes that "no member of our family has ever been a slave" (p. xxiii), it is possible that Sarah Madden's father or the father of one or more of her children was a slave. The author's lack of curiosity about the family's paternal ancestors makes an eloquent statement about the fragility of the precious legacy of freedom.

The author acknowledges Sarah's son, Willis, born in 1799, as the founding father of the family. Willis lived as a relatively prosperous free man of color in Culpeper County, working as a cobbler, blacksmith, distiller, and tavern keeper. The devastation of the Civil War confronted the family with hard times, followed by the humiliations of Jim Crow that embittered the author's father, a teacher for fifty-five years. A family of mixed racial ancestry, the Maddens "were caught in the middle. Whites wouldn't accept us because we had Negro blood. Negroes said we acted like we thought we were better than they were because we lived differently: We owned our farm and tried to get educated" (p. 152). The author singles out the end of segregation as "one of the most significant events that I saw in my life" (p. 164).

This volume adds fresh information to our limited knowledge of free people of color during the slavery era and of families like the Maddens since emancipation who were "no more 'black' than we are 'white.' And . . . we're too far from Africa, and of mixed blood for too many generations, to be Afro- (or African-) Americans" (p. xxv). This book makes clear that the Maddens could not be more American.

MICHAEL P. JOHNSON
University of California,
Irvine

JERRE MANGIONE and BEN MORREALE. *La Storia: Five Centuries of the Italian American Experience*. New York: HarperCollins. 1992. Pp. xx, 508. \$30.00.

This book by Jerre Mangione and Ben Morreale is a popular synthesis of the Italian-American experience in the United States, with all the merits and shortcomings that such endeavors generally display. Although the subtitle indicates a study spanning five centuries, the book is about the mass immigration of southern Italians, an event that started slightly over a century ago. The first four centuries are covered in the largely irrelevant thirty opening pages.

The book is organized into twenty-seven chapters, the first two dealing with the period preceding the mass immigration. Chapters 3 to 7 discuss southern Italy at the time mass emigration started. The following chapters describe patterns of settlement in American cities, the immigration of Italians to southern and western states, prejudice, the early linkage between organized crime and Italians, the jobs Italians embraced, their late involvement in politics, and the difficult process of assimilation. Chapters 24 to 27 indulge in a celebration of eminent Italian Americans in politics, business, and especially in the visual arts and fiction. In the final chapter, the two authors discuss the still ambivalent place of Italian Americans in their adopted country.

The book is not without merit. It was obviously timed to be released for the Columbus' quincentennial. It is directed to Italian Americans. One of the strongest assets of the book is the passion it exudes. Mangione and Morreale are totally absorbed by the narration of their ancestors' odyssey that forms the core of their ethnic identity.

The overall approach centers around the motif of victimization and success. Although victimized by nature in the Italian south, although oppressed by northern Italians who arrived in the south as "liberators" in 1860, although victimized in the adopted country, Italian Americans survived and the best among them achieved a degree of success that few other ethnic groups can claim. Lee Iacocca and Mario Cuomo are the two greatest heroes in a pantheon of new saints who have replaced the old village saints of the immigrant generation. The authors are particularly attentive to politics. Both in the Italian south and in the United States events are the result of human actions and interactions, which pit oppressors against oppressed. Although immigrants shared a common experience of oppression, their children finally achieved success, without joining the class of the oppressors, a conclusion that some minorities in American cities might challenge.

Intensely emotional, personal, political, and in the end uplifting, a narrative of this kind is less attentive to some criteria highly valued by professional historians. By emphasizing politics, the authors overlook the complexities of the social and economic dynamics that affected the Italian south in the late nineteenth

century, as the impact of large-scale capitalism made the traditional regional economy obsolete. Intensely emotional, the book fails to achieve the objectivity needed to place the Italian-American experience within a larger historical perspective. The victimization motif leads the authors to some astonishing statements unacceptable even in a book of popular history. For instance, in discussing efforts by the new Italian state to put down regional rebellions in the south after political unification, the authors state that "whole villages were burned to the ground on the mere suspicion that they might be providing food and shelter to the rebels" (p. 60). Finally, the Italian-American experience seems to occur in a vacuum. The United States is a distant background, to be retrieved only when the topic of discrimination is discussed.

The obvious question is whether a book like this is useful or not. On the one hand, we can reasonably argue that this book fosters a sense of ethnic identity, certainly badly needed among Italian Americans, especially among the young, who are ambivalent about the need to understand their past and confused by the subliminal family messages of an undesirable ethnic identity. On the other hand, we are aware that the cultivation of ethnic pride is not without shortcomings. Is any narrative reinforcing ethnic identity desirable and useful? Such narratives are usually passionate, but they should also strive for objectivity; they address the concerns of one ethnic group, but they should not be oblivious of complexities and broader issues. In ethnic studies we have not found the way to write histories that are both popular and scientifically accurate. Perhaps this is a reflection of our larger inability to find a resolution between ethnic and national identities. This book is a further reminder of an agenda that we, both as scholars and citizens, cannot escape for much longer.

DINO CINEL

City University of New York

ERIC C. SCHNEIDER. *In the Web of Class: Delinquents and Reformers in Boston, 1810s-1930s*. (The American Social Experience, number 22.) New York: New York University Press. 1992. Pp. xiii, 260.

Eric C. Schneider, himself a participant in the welfare system, uses his description of the evolution of key Boston juvenile institutions in the years from 1810 to the 1930s to argue that all these groups, despite seeming differences, shared a common goal aimed at reshaping the cultural values and behaviors of the lower class. These systems, which found youth especially appealing because they appeared malleable, failed because they did not address the significant structural economic issues underlying poverty. He identifies four groups: the religious and moral, the domestic, the manual fitness reformers, and finally the Progressives who brought "scientific" expertise

and especially psychiatry to bear on the issues of juvenile delinquency. All failed to focus on issues of class, power, and economic matters and, as a result, treated the symptoms, not the causes. Even the unique individuals who established reasonable programs ultimately failed since their efforts were hampered by the doctrines of limiting costs and of "less eligibility." The latter meant that those receiving aid could not get more than the working poor not getting aid. Even the juvenile court system, often viewed as a major change in the juvenile welfare system, was, according to Schneider, a part of the cultural reform movement and incapable of significantly reducing juvenile delinquency. He concludes the book with the argument that those involved in today's juvenile issues must address structural, not moral, issues.

Perhaps the most intriguing part of Schneider's book is the argument that the youngsters and their families touched by the juvenile homes, asylums, and courts often resisted the agencies and manipulated them to achieve their own goals. "Delinquents refused to be the passive 'beneficiaries' of reform, and they and their parents used social services in unexpected ways" (p. 1). Although Schneider reports a few cases that seem to support this contention, there is just not enough evidence to make this more than conjecture except in the cases cited.

Schneider addresses gender differences in treatment, especially noting the difficulty reformers had dealing with sexually active girls. Ethnic differences such as the Irish immigrants' acceptance of domestic service as opposed to the Italian and other immigrants' hostility are used to explain why some strategies failed as the groups served changed.

Contrary to most studies of Boston, Schneider plays down the conflict between Catholics and Protestants, choosing to focus on the periods of accommodation and cooperation rather than periods of great tension and even violence. He cites the new bourgeois Irish Catholics who were closer to bourgeois Protestants than to poor Irish. Common class interest allowed them to cooperate when dealing with the poor. "Poverty rather than Catholicism re-emerged as the distinguishing badge of deviance" (p. 126). But Schneider himself later notes that ethnic and religious rivalry reemerged, yet he discounts it as minor compared to midcentury conflicts.

Schneider's work clearly shows the positive influence of modern social history in his account of the juvenile institutions and agencies in Boston. He has carefully mined the primary sources and secondary works and written an interesting book. Perhaps historians working in other cities can provide evidence to support his contention that the clients of the juvenile system significantly used and reshaped it to satisfy their needs.

MIRIAM Z. LANGSAM
Indiana University-Purdue University,
Indianapolis

JACQUELINE JONES. *The Dispossessed: America's Underclasses from the Civil War to the Present*. New York: Basic Books. 1992. Pp. xiii, 399. \$25.00.

This book offers a thoughtful history of southern, largely rural, American workers. Using mostly anecdotal data, Jacqueline Jones explores the history of poverty in an effort to restore the dispossessed to their central place in the American past. Her major argument is both simple and controversial: she seeks to convince us that poverty has no color.

Broad as is the scope of Jones's effort, it is narrower than the title suggests. Beginning with an overview of southern poverty during the Civil War, the book turns to the lives of black and white agricultural and rural workers in the period from 1870 to 1930. The heart of the book lies in four long chapters that first detail the lives of African-American and white sharecroppers, and then demonstrate the continuity between these lives and those of the rural proletariat. Poor southerners, black and white, not only farmed the land on the worst possible terms but they also worked in extractive industries, producing phosphate and coal, harvesting and processing lumber and oysters, and became migrant laborers. Jones sketches the economic processes that led people to follow the harvests and illustrates the hard work that characterized their lives. These four chapters are followed by two less-satisfying accounts of what happened to southern workers in the exodus to the Midwest around World War I. Here, Jones contrasts what she characterizes as the individualistic attitudes of white Appalachian migrants with the collectivist stance of African Americans. The book closes with an assessment of the treatment of poverty in the postindustrial world.

The common themes that run through the lives of the largely male workers around whom this story rotates are hard work and the desire to escape from supervision. Jones seeks to defuse the old myth that poverty is a consequence of laziness. Her families searched tirelessly for work that would provide more than minimal sustenance. When they found it, their capacity to work long hours at backbreaking rates did not prevent employers from cheating them. Under the circumstances, the desire to be free of close constraints seems logical. Sharecropping was a desirable activity insofar as it left a worker with some autonomy. Families moved when the landowner became too intrusive. "Shifting," as this process was called, was an act of resistance. And, according to Jones, workers sought jobs in extractive industries in preference to those in factories because, in the words of one coal miner, "they don't bother you none" (p. 137).

Jones tells us that after 1910 black families moved to the Midwest to find the independence that had been denied them in the South. Far from reflecting the "roving disposition" of which southern landowners accused them, the northward migration was "a

continuation of their historic quest for freedom" (p. 206). Although both poor whites (who migrated in almost equal numbers) and African Americans did so out of urgent economic necessity, the latter were pushed by a larger set of political and moral aims as well. Still, the search for steady work at decent pay, education for the kids, and a home of one's own underlined the movement of blacks into cities, a move that was fueled, according to Jones, by a defiant and resourceful optimism. Here they were to be disappointed. Jones's final chapters suggest that discrimination at work and a politics of race have, in the end, denied many African Americans the opportunities they sought.

Jones makes a convincing case for the efforts of African-American families to make their way in the world and thus challenges any remaining believers in the dogma of African-American "laziness." Poverty is endemic to the culture, not to race, Jones suggests. But in seeking commonalities in the condition of poverty, Jones leaves unanswered some of the pressing questions she herself poses. In what sense does race constitute a divide in the experience of black and white sharecroppers? What are the historical forces of marginalization that engulf poor black and white people? What are the continuities in the politics of race? If our ability to ask these questions is enhanced by the lucid exposition Jones presents here, the book also points up the urgent need for historians to find answers.

ALICE KESSLER-HARRIS
*Rutgers University,
New Brunswick*

MARILYN IRVIN HOLT. *The Orphan Trains: Placing Out in America*. Lincoln: University of Nebraska Press. 1992. Pp. 248. \$27.50.

This is an important, intelligently argued, and well-documented history of one of the most unusual reform movements in American history: the removal of the children of poverty from eastern cities to rural areas of the United States, frequently in the West, between 1850 and 1930. As Marilyn Irvin Holt demonstrates, the child-removal movement relocated approximately 200,000 individuals, most of them children, a minority of them orphans, all of them from impoverished or abusive families, about 60 percent of them male, from the slums of major cities (especially New York, Boston, and Philadelphia) to rural areas in the West, the South and, to a lesser degree, upstate New York and New England. The basic premise of this movement, which was led by Charles Loring Brace's Children's Aid Society, was that even the most abused and exploited children could be redeemed and rendered into productive citizens if they were extracted from the immoral atmosphere of industrial society and placed within the supposedly salutary sanctuary of rural life.

Holt argues that the "placing out" movement was motivated by other concerns as well, especially the need for cheap and plentiful labor on the farms and within the small towns of the mid-nineteenth-century West, and the assumption by Brace and other reformers that the child of urban poverty could be "saved" if extracted at a sufficiently early age from the disease, death, and moral squalor that suffused the nineteenth-century industrial city. Holt effectively describes and analyzes the institutionalization of these assumptions and values. The operations of the Children's Aid Society, as well as the New York Foundling Hospital, the Boston Children's Mission, and the Philadelphia Women's Industrial Aid Association—all of which were involved in placing out—are discussed in depth and with subtlety, and include evidence about their uneven attempts to trace the fates of the placed-out children. Holt allocates less space to the actual experiences of both the displaced children (who hailed mostly from American-born and German and Irish immigrant families) and the families to whom they were entrusted. While Holt does contend that the consequences of placing out on the children were varied—some thrived in their new environments, some ran back to their former haunts, others were exploited by labor-starved westerners—more emphasis on what the placed-out children said and felt about their fates would have provided a firmer foundation on which to judge the actions and values of the reformers.

Perhaps the most important aspect of the book is Holt's attempt to explain the demise of placing out. She contends that a variety of factors coalesced to end the movement, including the evolving resentment of westerners about the "dumping" of eastern "undesirable" into their states. Even more significant, according to Holt, was a late-nineteenth-century shift in the approach to poverty, family abuse, and child welfare. The professionalization of social work, with its focus on the "objective" quality of social phenomena such as poverty, gradually replaced the "moralistic" orientation of reformers like Brace. Instead of what Holt calls the "missionary" zeal of midcentury reformers, who generally assumed that parents immured in a life bounded by poverty, alcoholism, and apathy were beyond social salvation and, therefore, only the still-impressionable children be saved through removal from that milieu, professional social workers later in the century routinely assigned the causes of these deficiencies to the objective social conditions (such as poverty, immigration, and unhealthy living environments) within which the family lived. From this perspective, the entire family, not just the children, should be "saved."

Holt might have delved deeper into the sources of this shift in the sensibilities of reformers. Beyond the differences in philosophy between Brace and someone like Jane Addams, is it possible that changing visions of family life, the increasing prevalence of mandatory education laws, and altered perspectives

of human potential also played vital roles in the demise of placing out? Nonetheless, this is a penetrating and deftly written history of a most unusual and largely forgotten reform movement that deserves the careful attention of social historians.

DOMINICK CAVALLO
Adelphi University

LOUIS C. HUNTER and LYNWOOD BRYANT. *A History of Industrial Power in the United States, 1780–1930*. Volume 3, *The Transmission of Power*. Cambridge: Massachusetts Institute of Technology Press. 1991. Pp. xxv, 596. \$50.00.

Louis C. Hunter began his study of industrial power in the United States in 1947. Two volumes of this history, dealing with water and steam power, were written before his death in 1984. This third and final volume was completed by Lynwood Bryant, who edited Hunter's manuscript and brought it to press with the support of the Hunter Memorial Publication Fund.

The focus of this study is the transmission of power and the changes in distributing energy that occurred in the twentieth century. Much of the book is taken up with an account of the development of electric power and its impact on industry. This was a versatile form of energy that brought new levels of productivity to the user. It freed industrial establishments from providing their own power and from the constraints of overhead belting or hydraulic pipes. It eased the burden on the worker and made the shop floor cleaner and safer. It was at the center of a new era of power transmission. Hunter concludes that "The transmission revolution made possible a new industrial revolution" (p. 315).

This paean to electric power does not mean that Hunter ignores other forms of transmission; he places electricity within the context of the search for a flexible power source that included wire rope, compressed air, and hydraulic systems. He notes, for example, that the November 1881 issue of *Scientific American*, which announced Edison's plans to build a central station system in Manhattan, also contained an article about a steam heat system under construction at the same time.

Whereas the two other volumes in this series each dealt with only one source of power, this book covers several types of power and its transmission. In addition to the lengthy section on electricity there are also chapters on mining, petroleum extraction, mechanical power for small enterprises, and the muscle power of men, women, and animals. Hunter's knowledge is encyclopedic and he writes with the same authority and erudition on matters as diverse as a dog-powered treadmill or the mighty hydroelectric scheme at Niagara Falls. Each system is explained clearly and the economic consequences evaluated as he blends the history of technology with economic history.

In his introduction Bryant sums up the deficiencies of the book: the limited scope, the lack of a strong organization, and the inability to make use of scholarship published after 1975. This might be, as he says, an incomplete study of American industrial power, but it is a wide-ranging and informative narrative that reflects Hunter's enormous knowledge and his understanding of the topic. This book is thoroughly researched and profusely illustrated. It meets the high standards of scholarship established in the two previous volumes and forms a fitting memorial to Hunter.

ANDRE MILLARD
University of Alabama,
Birmingham

LYSLE E. MEYER. *The Farther Frontier: Six Case Studies of Americans and Africa, 1848–1936*. Selinsgrove, Pa.: Susquehanna University Press or Associated University Presses, Cranbury, N.J. 1992. Pp. 267. \$39.50.

This book looks at the activities of six Americans who played important roles in Africa between 1848 and 1936. They were Thomas Jefferson Bowen, a converted hellion who set up the first American mission posts in Yorubaland (Nigeria) and tried to Christianize Muslims beyond the Niger River; Paul du Chaillu, explorer of Gabon and a popular writer on Africa; Charles Chaille-Long, soldier-explorer who worked for the Egyptian government in the Sudan and East Africa; Henry Shelton Sanford, a diplomat and lobbyist who played an important role in the Berlin Conference and in setting up the Congo Free State; John Hays Hammond, Cecil Rhodes's chief mining engineer in South Africa and a participant in the Jameson Raid fiasco which led to the Boer War; and Carl Akeley, taxidermist-hunter-naturalist who led five collecting expeditions in Africa and was influential in setting up Africa halls in American museums of natural history.

Thomas Jefferson Brown was one of the most colorful personages among those American missionaries who helped Christianize and agriculturally develop Black Africa, especially in Liberia, Nigeria, Cameroon, the Belgian Congo, Kenya, and Southern Rhodesia. Black Americans also played an honorable role in converting and educating Africans, primarily through black churches such as the African Methodist Episcopal church. American missionaries were especially good in providing practical education, that is, industrial arts and agriculture. American missionaries such as Bowen also did good work in linguistics and translations.

American traders, merchants, and entrepreneurs looked, mistakenly, to Africa as a treasure house of riches. One such person was Sanford, who was linked to Leopold II, king of Belgium and owner of the Congo Free State. To his credit, Sanford pushed for

free trade and African rule at the Berlin Conference; he opposed colonial occupation of Africa.

Another entrepreneur was Hammond, who started out as a mining engineer but soon became a mining entrepreneur for Rhodes. Hammond brought American mining engineers and mining technology to South Africa, the Rhodesias, and the Congo Free State. He linked American capital to South Africa as well. So strong was the American mining influence in Northern Rhodesia and the Congo that the British and Belgians feared an American takeover.

These biographical sketches expand the picture of American activities in Africa I published, most notably in *The United States and Africa: A History* (1984). Meyer's work is based on extensive research in archives and on a critical evaluation of primary sources and secondary works. These Americans played an important role in African history, but their work was also relevant to American history as well and especially the Americans' image of Africa.

PETER DUGNAN
Hoover Institution

ROBERT H. FERRELL. *Ill-Advised: Presidential Health and Public Trust*. Columbia: University of Missouri Press. 1992. Pp. xii, 205. \$19.95.

Robert H. Ferrell's slender volume devotes 100 of its 164 pages of text to a detailed narrative of the health of President Dwight Eisenhower, based primarily on written and oral testimony from Dr. Thomas Mattingly, Eisenhower's cardiologist. The Eisenhower material is remarkably detailed and mainly new. It indicates that Eisenhower's health was much poorer than the public was told or than previous writers have imagined. It is buttressed by brief accounts of the physical ailments and the White House cover-ups of those disabilities of presidents Grover Cleveland, Woodrow Wilson, Warren Harding, Franklin Roosevelt, John Kennedy, Ronald Reagan, and George Bush. Little of the material on these six men is new.

Ferrell, best known for his extensive work as an editor of historic documents, is an indignant author. He is especially critical of the cover-ups of Cleveland's operation for cancer of the mouth, Wilson's stroke, Roosevelt's obvious approaching death during the 1944 presidential campaign, the severity of Eisenhower's 1955 heart attack, and Kennedy's Addison's disease. These cover-ups, in his view, constitute "one of the most distressing developments in American national politics . . . It is a deplorable situation . . . [Voters] need to know, down to the smallest personal detail, when the subject of their trust is incompetent to carry it out" (p. ix).

The new material on Eisenhower leads Ferrell to conclude, "Eisenhower probably should not have run for the presidency in 1952" (p. 53), because his heart had already been weakened, and surely he should not

have run for reelection in 1956, because his statistical chance of surviving for four years after his 1955 heart attack was not good. Ferrell's judgment inevitably raises the question, would he rather have a sick Eisenhower or a healthy Richard Nixon (or Adlai Stevenson) as the national leader at the height of the Cold War? In any case, while Ferrell is convincing in his description of Eisenhower's heart and stomach problems, he does not convince me that the problems hampered Eisenhower's performance.

Ferrell is critical of the methods used for selecting a vice-presidential candidate in 1944, when Democratic Party leaders dumped Henry Wallace and brought in Harry Truman, "a man [Roosevelt] hardly knew," and in 1956, when he asserts that Eisenhower showed a "similar unconcern" about Nixon's place on the ticket (p. 117). Regarding 1956, the fact is that Eisenhower put more time and energy into the selection of his vice-presidential candidate than on any other political problem.

The book provides an opportunity for a thorough discussion of the Twenty-Fifth Amendment and its implications, but surprisingly Ferrell has little to say on the subject. He worries, rightly, about the gray areas. Suppose the president is mentally unbalanced, as Wilson was and "as one might argue was true of President Nixon" (p. 162)—an assertion for which Ferrell supplies no supporting evidence whatsoever—but still refuses to leave the Oval Office? What then? The answer apparently is, God help the Republic.

Ferrell concludes with a recommendation to require Senate consent to the appointment of the president's personal physician, as is already done for the president's economic advisors. Given the incompetence he describes of Wilson's and Eisenhower's doctors, he has a case.

STEPHEN E. AMBROSE
The Eisenhower Center
University of New Orleans

THOMAS E. BURKE, JR. *Mohawk Frontier: The Dutch Community of Schenectady, New York, 1661-1710*. Ithaca: Cornell University Press. 1991. Pp. xvii, 252. \$32.50.

Schenectady is best known during the colonial period for the French raid in the winter of 1690 that destroyed the town. While Thomas E. Burke, Jr., scrutinizes the raid closely, he also traces the evolution of this frontier settlement through fifty years after the initial founding in 1661.

Schenectady was created to guard the Dutch fur trade against the encroachments of the English and to provide land for the growing number of Dutch settlers around Albany. It never became a fur trading center because the merchants of Albany regarded it as a potential rival that might contest it for control of the fur trade. They sought, and obtained, protection in a monopoly granted by the Dutch and then later by

the English governments. The new village, however, situated west of Albany on one of the main routes into town was always a magnet for Indians with fur and a permanent clandestine trade resulted. Fur was only one aspect of the town's economy, and its importance declined as the years passed and the traders retreated westward and developed new routes and methods of trade. Farmers, who increased slowly but steadily in numbers, exploited the rich bottom lands along the Mohawk River. By the early eighteenth century the produce of the farms far exceeded the fur trade in value.

The farmers were overwhelmingly Dutch, and while other ethnic groups were present, especially Native Americans, none ever challenged the Dutch for control of the town. It cannot be said that the Dutch created a community in Schenectady. The town even lacked control over its own governance; it did not become a borough until 1765 and was not incorporated until 1791. Once again Albany was the villain in keeping Schenectady from acquiring the institutional framework it needed to properly control its own affairs. Local problems were handled by the town proprietors and, because they controlled the land, which was the most frequent source of disputes, they tended to win any debate unless they became divided among themselves. Such was the case in the aftermath of Leisler's rebellion when Leislerians and anti-Leislerians fought for power and produced divisions that were heated and bitter and continued long after the end of the insurrection.

Due to the destruction of records during the French raid and other vicissitudes, few documents have survived, thus making the task of the historian difficult. The staples of community studies, such as family reconstitution or the transmission of property over generations, are either impossible or can only be done in attenuated form. Indeed, at times one feels that if this book were a biography there would be rather more "times" than "life." A great deal of effort is spent considering events beyond the bounds of the community and, while they affected the life of the town, the amount of detail devoted to them is too great. Nevertheless, what can be known about Schenectady's early history as a community can be found here.

ROBERT C. RITCHIE
Huntington Library
San Marino, California

MERRIL D. SMITH. *Breaking the Bonds: Marital Discord in Pennsylvania, 1730-1830*. (The American Social Experience Series, number 21.) New York: New York University Press. 1991. Pp. xv, 225. \$40.00.

Merril D. Smith has produced a well-documented account of marital disputes, divorce cases, and conflicts over spousal support in Pennsylvania from 1730 to the 1830s, a period encompassing passage of the

Pennsylvania Divorce Act of 1785 and numerous subsequent reforms. The book contains useful data and lively extracts from newspapers, diaries, alms-house dockets, and legal documents, although its most interesting and provocative observations appear as sidelights to the central theme.

The thesis of the book is that rising expectations about companionate marriage resulted in new sources of marital strife and new ways of resolving conflict in the late eighteenth century. Smith shows that the divorce laws of 1785 and later gave unhappy couples new options over the period, but her cases reveal less change in the type of disputes, language of complaints, cited grounds for divorce, and provision for separated women than one might expect from this thesis. In fact, "couples faced many of the same problems in the 1830s as in the 1730s. Typically, they quarreled over money, sex, drinking, abusive behavior, and in-laws" (pp. 4-5). Although Smith argues that women increasingly expected "reciprocal relationships" with their husbands, desertion remained the main grounds for divorce alleged by women throughout the period (and well beyond), while cruelty was alleged less often by nineteenth-century women than by eighteenth-century ones (pp. 37-41).

Attempting to negotiate this gap between the changes in marital prescriptions or expectations and the continuity in patterns of marital disputes, Smith ends up qualifying her assertions so extensively as to seriously undercut them. As she sums up her findings: "Conflicts ensued in some cases when husbands retained their patriarchal viewpoints while their wives embraced newer companionate forms of marriage, and in other instances emerged when husbands failed to take charge as the master of the house" (pp. 179-80).

Important terms, such as patriarchy, go undefined, and it is not always clear what is distinctive about the period under review. What precisely did women of the period expect from "reciprocal relationships," and how did this definition differ from definitions of mutuality or reciprocity in earlier and later generations? Was there anything unique to this time period about the fact that men and women entered marriage with "distinct gender differences" in their views (p. 182)?

Nevertheless, there are several original arguments and observations to be found here. Chief among them is Smith's conclusion that the marital conflicts she studies refute the notion that a cult of family privacy had developed by the first third of the nineteenth century. In the most interesting and well-documented part of the book, Smith convincingly establishes the integral involvement of neighbors and other community members in monitoring, mediating, evaluating, and choosing sides during all phases of marital disputes. Had the book been recast around this theme, Smith would have been better able to position herself in relation to current debates over

family history and the concept of public and private spheres.

Other intriguing points include Smith's suggestion that the "marriage trauma" described by Nancy Cott for the early nineteenth century (*The Bonds of Womanhood: "Woman's Sphere" in New England, 1780-1835* [1977], 80) was evident as early as the mid-eighteenth century (p. 61). Smith also mentions that while republican principles led many women to describe unsatisfactory husbands as "tyrants" between 1785 and 1815, such republican rhetoric disappeared from marriage literature in the nineteenth century (pp. 51, 55).

The book is useful for its evidence that the protections of both patriarchal marriage and early divorce reform were very fragile indeed. Women and children frequently faced violence and impoverishment, both within marriage and after separation. Smith also provides readers with a nice sense of the community values and neighborhood involvement surrounding marital norms and conflict. In its central thesis, however, the book breaks little new ground. Perhaps in a future work Smith will elaborate the other historical points and theoretical suggestions that are raised but not fully developed here.

STEPHANIE COONTZ
Evergreen State College

RICHARD C. SINOPOLI. *The Foundations of American Citizenship: Liberalism, the Constitution, and Civic Virtue*. New York: Oxford University Press. 1992. Pp. 215. \$32.50.

For some time historians of ideas have become accustomed to regarding political theorists engaged in explicating the master texts of political thought as co-workers in the same enterprise. Richard Ashcraft, John Dunn, and Quentin Skinner have given a historical turn to the subject that makes their work methodologically indistinguishable from that of a historian like J. G. A. Pocock. Richard C. Sinopoli is striking out in a new direction from this tradition. He is less interested in a fully historicized reading of the master texts than in entering into a dialogue with them to illuminate the problems of the present. The problem that particularly concerns him is the character of American citizenship. He takes seriously complaints about the decline of civic commitment in modern liberal democracy, and he proposes to go back to the master text of the founding of the republic, *The Federalist*, and the debates over the ratification of the Constitution to explore the founders' ideas and assumptions about citizenship. In doing so he is led to explore the intellectual sources James Madison and Alexander Hamilton drew on and to consider the master texts of British political thought in the seventeenth and eighteenth centuries, particularly Hobbes, Locke, and Hume.

I suspect that historians will be more challenged than informed by this book. To the degree that they

are committed to understanding texts in historically specific contexts, they are going to feel frustrated by Sinopoli's approach. His interpretations are often controlled less by what the texts might have meant at the time than by what he thinks from his perspective they are required to have meant if they are indeed to be treated as master texts, that is, texts that solve at least some of the problems they seem to raise. Historians are also going to have difficulty with Sinopoli's perception of what problems the texts he discusses actually address. For instance, he considers the ratification controversy to be essentially about citizenship. I suspect students of the period will be more impressed by the novelty than the cogency of this claim and I doubt that a perusal of his argument is going to go far in reconciling them to his point of view.

One aspect of Sinopoli's analysis will seem familiar to historians. He underscores the tension between two interpretive approaches that have emerged in attempts to understand this period, one focusing on liberal contractualism and the other on republican civic virtue, and he uses that tension as the matrix of his analysis. One might even argue that his dialogue is as much with these paradigmatic interpretations as with the master texts he examines. Certainly Sinopoli comments on each text's "liberal" and "republican" dimensions. Those who believe they have a stake in this debate will undoubtedly want to examine Sinopoli's argument. But I doubt if the partisans to this controversy will be consoled by what they find. Although Sinopoli's presentist agenda suggests an essentially republican reading of his material, an expectation which he actively encourages in his introduction (pp. 4-5), he ends up emphasizing the liberal contractarian orientation of both Federalists and Antifederalists in his interpretation of the ratification controversy. Those heavily invested in stressing the classical republican aspects in revolutionary thought, however, will have little trouble discounting the significance of his findings as the predictable by-product of the essentially contractarian situation the ratification controversy necessarily was. Which is to say that this book is unlikely to have a major impact on our historical understanding of the founding, despite the considerable ingenuity in argumentation displayed in its pages.

RICHARD BUEL, JR.
Wesleyan University

JOHN PHILLIP REID. *Constitutional History of the American Revolution: The Authority to Legislate*. Madison: University of Wisconsin Press. 1991. Pp. ix, 495. \$35.00.

Like all the many recent books by John Phillip Reid, our most obviously erudite historian of late-eighteenth-century Anglo-American constitutionalism, this volume is less than the sum of its parts. It is full of niceties about its announced subject. And they are avowedly the niceties of the lawyer taking to task the

"methodology" of most historians of the American Revolution, who fail to give us, says Reid, an authentic history of what was mostly a legalistic dispute. No reader of the two earlier volumes in this series can have failed to notice Reid's special plea for "the authority of law" in explaining the revolution. But lest anyone has missed his point, Reid has now decided to add a hitherto unannounced fourth volume; it will be subtitled *The Authority of Law*.

Reid is right about the fact that most historians of the revolution have a great deal to learn from him; but we might well wonder why he makes it so hard. Within and among the volumes in this series, he repeats himself pointlessly and often. And he weakens his best arguments through a scheme of exposition that makes a fetish of fragmentation, especially by means of cross references to his own work. But what he has to say, however poorly said at times, is so important, and so scrupulously documented, that no responsible historian of the revolution can afford not to comb through this volume, indeed, all of Reid's recent work, for the bounty of information and insights it contains.

In this short review I can but try to intimate this bounty, in hopes of convincing prudent and diligent students of the revolution that this volume is obligatory reading. Here, more subtly than ever before, Reid argues, "It is not necessary to contend that the motivations of colonial whigs were solely constitutional. All that is claimed is that their constitutional strategies were consistently forensic" (p. 310). And Reid supports this claim by briefing in detail the legalistic constitutional arguments made by and for, respectively, the colonists and the imperial government. Both sides, he shows us, were caught up in a constitutional tragedy, because both sides truly, albeit variously, cared about a tradition of English common-law constitutionalism that had by the 1760s grown "ambiguous" and indeterminate at best, indeed, evidently contradictory. The colonists still held firmly to seventeenth-century Whig principles as safeguards against the *summum malum* of "arbitrary" government—power unchecked—while the imperial administration and Parliament no longer had much use for those principles after embracing the new constitutional ideology of parliamentary sovereignty, a constitutionalism of "power," residing solely in Parliament, on the authority of the mythic legacy of the Glorious Revolution.

Such is the grand theme of all the volumes thus far in Reid's series. What distinguishes this volume is, above all, its intensive analysis of the forensic arguments on both sides that arose out of the provocative assertion of parliamentary sovereignty in principle in the Declaratory Act of 1766, and then in the Coercive Acts of 1774, which put the principle into effect, and, from the perspective of conflicted Anglo-American constitutional law, made civil war virtually inevitable.

There is a profound latent drama in the locutory events that unfolded between these signal acts of

Parliament in 1766 and 1774. But Reid emphatically eschews a sense of drama. Instead, he parses, like a good lawyer, all the issues he can spot that were presented by the constitutional case at bar—parliamentary authority to legislate in all matters whatsoever—and by the problematics of constitutional theory that made and still make the case so hard to grasp and resolve. Reid is proudly a historian second and a lawyer first, one steeped in the libertarianism of eighteenth-century common law. He is desperately trying to get twentieth-century historians (distracted by matters economic, social, religious, political, and nationalistic) to pay attention to what colonial American Whigs said they cared about most of all: the law.

STEPHEN A. CONRAD
Indiana University,
Bloomington

ROBERT LECKIE. *George Washington's War: The Saga of the American Revolution*. New York: HarperCollins. 1992. Pp. ix, 672. \$35.00.

This book is about people. Brave and determined Americans motivated by a commitment to individualism and a love of liberty struggled for independence against incompetent British bureaucrats and an inept military leadership undercut by quarreling generals and inflexible tactics. Less an account of "George Washington's war," this is a history of the actions and motives of the British and American military leadership during the American Revolution.

Men, not political theory or long-term social forces and economic factors, make history in this study. Lively character portraits define personality and motive for virtually every major British and American military figure in the war for colonial independence. Washington, Richard Howe and William Howe, Henry Clinton, Charles Cornwallis, Horatio Gates, and Charles Lee all cast lengthy shadows across the pages of this book. Robert Leckie credits eighteenth-century ideas about limited war rather than a desire for colonial reconciliation for the Howe brothers' failure to end the rebellion quickly. Washington emerges as a confident commander only after his capture and incompetence removed the former British general Charles Lee from the scene. Horatio Gates gets no credit for the victory at Saratoga and all the blame for the disaster at Camden. Clinton and Cornwallis, meanwhile, seem to earn the fate owed commanders more concerned with themselves than their mission.

Curiously, Benedict Arnold's career ties much of the narrative together. A hero at Quebec and Saratoga, unappreciated by Congress, and unfairly victimized by the politics of post-occupation Philadelphia, Arnold assumes a tragic persona. Driven to consider treason by a loyalist wife, Arnold acted only after both Congress and his commander-in-chief failed him. Had Gates credited him for the victory at

Bemis Heights, had Congress fairly promoted and compensated him, and had Washington expeditiously convened a court martial to clear his record, Arnold, Leckie implies, would have taken a well-earned place among the heroes of the revolutionary war.

Leckie takes little notice of the impact of the war on other than its most prominent military leaders. The home front is generally ignored, except to note British atrocities. Life in the ranks receives only cursory treatment. Leckie recognizes but never explains why home and family often commanded a greater loyalty than country for many soldiers. Myths, stereotypes, and inaccuracies also mar Leckie's account. German soldiers are invariably crude and their leaders capricious; Native Americans are superstitious and cruel; British officers are inflexible and licentious; "feudal lords" (p. 125) dominate southern society; Thomas Jefferson is an "uncompromising pacifist" (p. 124); and Benjamin Franklin is denied a wife. American soldiers love liberty and serve courageously and compassionately. Finally, one comes away from Leckie's book with the impression that individual-minded, rifle-bearing patriots won their independence by overwhelming well-drilled but inflexibly trained redcoats burdened with muskets that could not be aimed.

Readers seeking lively portraits of the revolutionary war's military leadership will find this book enjoyable. Those looking for an account based on the most recent scholarship will be better served by Robert Middlekauff's *Glorious Cause: The American Revolution, 1763–1789* (1982).

LAWRENCE DELBERT CRESS
University of Tulsa

JOHN FERLING. *John Adams: A Life*. Knoxville: University of Tennessee Press. 1992. Pp. x, 535.

This is a wonderful biography of John Adams—more revealing of his personal qualities than Peter Shaw's *The Character of John Adams* (1976), more integrative of his public and private life than Page Smith's *John Adams* (2 vols., 1962), and now fully faithful to the abundant source materials on the Adams family in print and on microfilm. At 535 pages it is about as long as a manageable biography can be, and yet it narrates and assesses with some fullness an active, incessantly articulated life of ninety years. From 1753, when John Adams as a Harvard College junior made the first (surviving) entry in his diary, until his last utterance the day he died (July 4, 1826)—seventy-three years—he lived, and recorded, a very busy inner and outer life. Then, keeping in mind that later generations of his family, including eminent statesmen and historians, preserved his papers carefully in archives that never suffered serious loss from fire, theft, or neglect, and that these papers are now cared for and being published according to the highest standards of the historical profession, one sees the

enormous task and opportunity before the biographer. John Ferling has been faithful to both the task and the opportunity.

By inserting short sketches of the other leading founders, Ferling places Adams carefully among them. Adams always admired and appreciated George Washington, and he generally agreed with his policies as president, although Adams envied and sometimes scorned the popular adulation of a man he considered less profound than himself. Adams so disapproved Benjamin Franklin's casual, compromising tendencies and manipulating personality that he was almost blind to the older man's manifold contributions to the American Revolution. Thomas Jefferson was a true soul mate and colleague in revolution and nation-building, but his clever phrase-making and more pleasing, magnetic personality again filled Adams with envy. (Serious scholars though they each were, one gathers the impression that Adams was perhaps even more learned than Jefferson.) Adams's contempt for Alexander Hamilton rested on moral, personal, as well as political grounds, while the coolness and lack of real understanding that characterized the New Englander's relationship with James Madison (although Adams's approval increased considerably during Madison's presidency) seems to have rested on regional and personal biases more than anything else. Altogether Ferling helps us to see Adams's personal qualities and political contributions by comparing them insightfully with those of his colleagues.

The best feature of the book, however, is its delineation of Adams's preeminent contribution as a revolutionist and nation-builder. From his earliest disagreements with British rule in Massachusetts in the early 1760s until his hearty approval of the national Republican policies of the Madison and James Monroe administrations, and that of his own son in the 1820s, he kept steadily in view his vision of a genuinely independent, thoroughly republican, wisely governed, and morally guided nationhood. This led him to be a "colossus of independence" in 1775–76, guided his insistence on American rights in the peace negotiations of 1781–83, undergirded his approval of the constitution of 1787, led to his signing of the Alien and Sedition Acts, and kept him from provoking an unnecessary and potentially disastrous war with France in 1799. His intense commitment to the founding and proper direction of the new United States led him not only to despise partisanship and narrow-mindedness in others but also, more remarkably, to be himself always devoted to the public good. In making that point clear this biography achieves its basic excellence.

RALPH KETCHAM
Syracuse University

HARVEY FLAUMENHAFT. *The Effective Republic: Administration and Constitution in the Thought of Alexander*

Hamilton. Durham, N.C.: Duke University Press. 1992. Pp. x, 314. \$32.50.

In 1834, James Madison told Nicholas Trist how he and Alexander Hamilton, his great coadjutor in the struggle for ratification of the Constitution, had so quickly become political antagonists under the new federal regime. The British constitution had always been Hamilton's ideal, Madison recalled, and as secretary of treasury he had sought to "administer the Government . . . into what he thought it ought to be" (p. 80). Harvey Flaumenhaft does not challenge Madison's assessment. His goal instead is to demonstrate that Hamilton's concern with "efficacious administration" proceeded from his determination to make republican government work: "The people are the beginning and end of good government—but between the source and outcome operates that organization of means which is administration" (pp. 69–70). If Hamilton invoked Britain as the best model of a well-administered state, "monarchical" means were always designed to serve republican ends.

Flaumenhaft's crucial move is to discuss Hamilton's ideas about administration within the framework of his principled commitment to republican government. Hamilton may have been skeptical about the new nation's prospects, particularly after his Republican opponents finally ascended to power. But, as he wrote Gouverneur Morris in 1802, "I am still labouring to prop the frail and worthless fabric" (p. 194). It was the "propping" that signified most here, Flaumenhaft suggests, not Hamilton's derisive comments about the American constitution. Indeed, his continuing efforts to construct administrative props to popular government constitute the best evidence of his fundamental commitment to republicanism.

The bulk of Flaumenhaft's book consists of glosses and commentary on Hamilton's ideas about the efficient organization of the executive and legislative branches, and about the particular need for an independent judiciary in a popular regime. Hamilton's genius was to see that the "partitioning of governmental power" could promote "governmental efficacy" even while protecting the people from "governmental oppression" (p. 65). The challenge was to differentiate the parts of the system in a way that would not compromise, but would instead secure and strengthen, the whole. The problem with federalism was that the state governments themselves constituted "wholes," always potentially in conflict with the central government. It was differentiation within the federal government, with the president providing "unity," the Senate "duration," and the judiciary "independent judgment," that alone could counteract the powerful centrifugal tendencies of a responsive popular representation in the House of Representatives.

Historians will find Flaumenhaft's approach to Hamilton's political thought somewhat disconcerting. Flaumenhaft culls the vast corpus of Hamilton's writ-

ings for relevant materials, with scant regard for political or polemical contexts. Having established that Hamilton's concern for efficacious administration is grounded in principle, Flaumenhaft treats everything Hamilton ever wrote as more or less reasoned discourse, fit material for the great treatise on political science that he never wrote. History-minded scholars thus will learn little about the development of Hamilton's ideas, or about the occasions that provoked them. But they will learn something important about the principles and premises of Hamiltonian republicanism.

Flaumenhaft's insistence on Hamilton's thoughtfulness is a valuable corrective to the images of Hamilton as modernizing technocrat, or as reactionary reincarnation of Robert Walpole, the epitome of British corruption. Flaumenhaft shows that Hamilton rejected the "ancient ferocity" of classical republicanism; as a forward-looking, "modern" republican, "he joined with those humane men of enlightenment who . . . worked for a society imbued with the spirit of commerce and governed in accordance with the natural rights of mankind" (p. 33). Notwithstanding its unapologetic antihistoricism—or perhaps because of it—this book makes a valuable contribution to the current debate over the sources and implications of early American political thought.

PETER S. ONUF
University of Virginia

KENNETH A. SCHERZER. *The Unbounded Community: Neighborhood Life and Social Structure in New York City, 1830–1875*. Durham, N.C.: Duke University Press. 1992. Pp. xviii, 356. \$34.95.

Despite its conceptual elusiveness, the neighborhood figures as an important component of urban life. Images of the neighborhood, whether scholarly or popular, range from nurturing havens to contested terrain, from points of individual or group definition to meaningless units of urban life. The clashing visions and meanings associated with the neighborhood intrigued Kenneth A. Scherzer, and this book represents his attempt to trace the roots of the urban neighborhood and explore the historical validity of these images.

Focusing on New York City between 1830 and 1875, Scherzer examines the emergence of the urban neighborhood, its evolution as a residential reference point, and its meaning for mid-nineteenth-century urbanites. He begins with an analysis of the historical literature on the nineteenth-century neighborhood. According to Scherzer, what flaws these studies, regardless of ideological perspective or topic, is the tendency to romanticize the neighborhood and freeze it in time. To avoid the trap of the "neighborhood of nostalgia" (p. 1), Scherzer turned to the work of urban sociologists and anthropologists. From them Scherzer learned that the combination of transiency

and heterogeneity did not always produce social dislocation or fragmentation. Instead, urbanites drew both from spatial identification as well as from broader, overlapping communities in their quest for a sense of place in a continuously evolving city.

The work of urban sociologist Albert Hunter provided Scherzer with a framework within which to explore a more "multilayered picture" (p. 13) of the urban neighborhood. Hunter argued that the historical examination of the neighborhood would be enhanced by an analysis of three factors: ecological, symbolic, and social. These factors provide the structure for the bulk of Scherzer's study. Part 1, "The Ecological Neighborhood," focuses on the demographic experience of four sample wards. Part 2, "The Symbolic Neighborhood," examines the varying perceptions of community operative in these wards during the mid-nineteenth century. And Part 3, "The Social Neighborhood," explores the dynamics of formal and informal interaction among these New Yorkers and the relationship of this interaction to definitions of space.

What Scherzer finds is that mid-nineteenth-century New Yorkers experienced the city during a period of urban reorientation. Under the impact of population growth, transportation innovations, and industrialization the city began a process of transformation that would ultimately give late-nineteenth-century cities their distinctive differentiated form. As urbanites adapted to this process they developed a new vocabulary that allowed for both residential identification and participation in broader, aspatial formal and informal networks. In short, city dwellers lived in unbounded communities that allowed them to come to terms with urban change.

Scherzer's study represents an important advance in the approach to neighborhood studies. At times his book becomes overburdened with information. His text, although not his endnotes, is too narrowly focused on New York City. Nonetheless, his study moves beyond the propensity of historians to view the neighborhood apart from the dynamics of the larger city of which it is a part. We need more studies that explore the interrelationship between the neighborhood and the city over time.

PATRICIA MOONEY-MELVIN
Loyola University of Chicago

VICTORIA E. BYNUM. *Unruly Women: The Politics of Social and Sexual Control in the Old South*. (Gender and American Culture.) Chapel Hill: University of North Carolina Press. 1992. Pp. xiv, 233. Cloth \$34.95, paper \$12.95.

In recent years historians have begun to delineate the middling and poorer free people—black and white—of the antebellum South and their social and economic worlds. These scholars have primarily studied the upper part of these social groups, but Victoria

E. Bynum investigates people whom society deemed more marginal or even deviant. Her title catches her focus on those women whose behavior, whether private or public, challenged southern mores and irritated the social order. Examining the lives of these little-known women reveals a South full of conflict where women's actions were evaluated according to standards peculiar to each class, race, and gender.

Bynum examines women's status and behavior in three Carolina Piedmont counties (Granville, Orange, and Montgomery) and provides a convincing picture of these communities and their social and economic differences. Relying heavily on public documents, especially governors' papers and petitions, court records of criminal actions, civil complaints, and divorce suits, and on manuscript returns of the federal census, she follows the lives of three different groups of women who came into contact with the authorities: those who through divorce actions challenged their husbands' authority and behavior, those who violated sexual norms, and those who resisted the Confederate government during the Civil War. Although the second and third groups mainly contained relatively poor white and black women, the first group also included well-to-do ladies.

Bynum presents a powerful exposé of the seamy aspects of antebellum southern society. Whether discussing the antifemale bias of divorce and property laws, the persecution of women who crossed the color line, or the bravery of ordinary women in standing up to the Confederate government, Bynum sympathetically chronicles her subjects.

What holds this examination of somewhat disparate groups of women together is Bynum's belief, much like Elizabeth Fox-Genovese's, that the patriarchalism of the southern ruling class demanded the subordination of all women in order to protect slavery. Bynum argues that these southern authorities were particularly interested in securing control over females whose reproductive capacities neither added to the slave labor force nor produced heirs for well-to-do families. Thus, the author presents the rigid code of conduct inflicted on women as springing from the needs of a slave society rather than from any religious, intellectual, or moral beliefs. Because other contemporary societies enforced similar codes of behavior, some readers may doubt this connection between slavery and the actions of the southern authorities. Indeed, Christine Stansell's study of poor women in New York City seemed to yield similarly intrusive and punitive officials (*City of Women* [1986]). Yet even readers uneasy with Bynum's overarching framework will appreciate her nuanced depiction of the elaborate networks of kin and neighborly relations in the Piedmont and will find her study immensely informative and persuasive about social groups in the South that heretofore have been little understood.

JANE TURNER CENSER
George Mason University

BILL CECIL-FRONSMAN. *Common Whites: Class and Culture in Antebellum North Carolina*. Lexington: University Press of Kentucky. 1992. Pp. xi, 274. \$34.00.

This book, a study of the culture of "common whites" in antebellum North Carolina, is useful reading for anyone wishing to understand the cultural complexities of the Old South. Using Governors' and Legislative Papers, Federal Writers' Project Papers, federal censuses, and many other primary sources, Bill Cecil-Fronsman carefully weaves in elements of class, race, religion, and community that framed the world of these mostly landowning, nonslaveholding farming people who comprised the Old South's majority. Common white culture, a blend of Old World traditions and New World experiences, included firm ideas about the proper structure of community and family relations. It displayed a strong suspicion of strangers and "strange" ideas, and it accepted violence as an appropriate means for upholding personal honor and community order.

The New World's abundant land and the American Revolution eroded traditional notions of deference, reinforcing an egalitarian ethos that proclaimed common white men equal to men of wealth and power. As Cecil-Fronsman argues, however, equality did not imply equal material status, but rather simply "the absence of dependence upon others. Freedom resulted from owning productive property" (p. 50).

Although no class revolution occurred in the antebellum South, common whites, according to Cecil-Fronsman, did not ape the behavior or even necessarily emulate planter-class status. Conflicts over individual rights and class privileges occurred frequently, but they generally did not produce a broad class consciousness that challenged the fundamental distribution of power. Bonds of family and community and shared ideas about appropriate political, social, and racial relations diminished class divisions among whites.

Cecil-Fronsman effectively argues that egalitarian political ideas fed the growth of a virulent racism that enabled planters to achieve class consensus. With notable exceptions, particularly Quakers and Wesleyan Methodists, common whites accepted African-American slavery as essential to their own freedom and independence. Slavery, justified by claims that African Americans were intellectually and morally incapable of exercising personal rights, represented an "abject and perpetual dependency" (p. 85) reserved in America for the unfit. Common whites believed that their own freedoms depended on the subjection of blacks.

Class consensus among white southerners, however, was never absolute. Whereas most common whites clung to racial distinctions because of their own lowly class, some forced alliances with African Americans. As Cecil-Fronsman points out, "although common whites did not like black people, they also did not necessarily like rich people" (p. 93). Common

whites and African Americans often crossed the color line when it served their mutual purposes. They traded goods, socialized, and sometimes cohabited. Some southern whites even assisted runaway slaves. The poorest whites, often themselves outcasts, were most likely to blur racial boundaries. Lower-middle-class whites, especially those "strainers" who hoped to climb the social ladder, were least likely to do so.

Although this book includes women, its gender analysis is inadequate. Except for a section on family relations, Cecil-Fronsman subsumes gender within analyses of class and race even when women are central figures. For example, he analyzes seventeenth and eighteenth-century English love songs in terms of class, but not gender relations. Similarly, interracial sexual relations are analyzed in terms of power struggles between white men and black men, but not between women and men.

An epilogue considers the impact of the Civil War on the world of common whites. The inner civil war that erupted between nonslaveholders and the Confederacy in many areas of North Carolina attested to the independence of yeoman culture, but the fears of black equality that led most common whites to ally with the old planter class during Republican Reconstruction revealed the depth of their racism and resentment of outsiders.

VICTORIA E. BYNUM

Southwest Texas State University

ERIC H. WALTHER. *The Fire-Eaters*. Baton Rouge: Louisiana State University Press. 1992. Pp. xviii, 333. Cloth \$39.95, paper \$12.95.

This treatment of nine prominent fire-eaters will quickly become a standard reference for southern historians, though for its separate chapters rather than for its conclusions. Eric H. Walther describes all but two of these fire-eaters in individual sketches because they "interacted infrequently if at all" (p. 7). Blending biography and intellectual history, the sketches are more descriptive than analytical, but they provide solid introductions to their subjects.

Walther chose these fire-eaters to illustrate the secession movement's "unity and diversity" (p. 6). Five of them, however, were closely associated with South Carolina: Robert Barnwell Rhett, the state's senior secessionist; Lawrence Keitt and William Porcher Miles, natives who made their careers there; and Louis Wigfall and James D. B. De Bow, natives who migrated as adults to Texas and Louisiana. The latter four were just reaching adulthood when Rhett staked his claim on radicalism in the 1830s. Alabama's William Lowndes Yancey was connected to South Carolina through family, law studies, and a law partnership with Rhett's cousin. Three older men, less directly linked with the Palmetto State, complete Walther's lot: Edmund Ruffin and Beverley Tucker of Virginia, and Yankee-born John Quitman of Mississippi.

Walther's sketches are nicely drawn. He is especially good at capturing eccentric personalities (like Yancey's, for example), and he places each man's ideas in social context. In contrast to the sketches, however, Walther's evaluation of the fire-eaters' common ideas is superficial. His brief conclusion describes them as defenders of "a particular ideal of republicanism" (p. 299) threatened by northern power and as agitators for a southern polity untainted by party politics or class warfare. Beyond these generalities, ideological diversity led to their ultimate success.

Walther asserts the significance of diversity more strongly than he proves the case, however, and his argument has logical flaws. Some major intellectual issues are neglected. He barely touches the critical subject of fire-eaters' nationalism, and he inadequately distinguishes their republicanism from many other forms of the creed. He notes variations on liberal and conservative themes, but he fails to assess such commonly felt tensions as those between progress and stability, or liberty and slavery. By slighting issues such as these, Walther may overlook underlying unities among radical southerners. Moreover, even had he demonstrated conclusively that diversity outweighed unity among these men, Walther does not account for the fire-eaters' success. He defines his selection criteria poorly, so that his subjects cannot be readily separated from other aggressive politicians who influenced secession. And, when all is said and done, nine character sketches cannot possibly explain the success of a broad movement that won popular support across the South.

Although marred by the claim that it has found the "key to their success" (p. 299), this treatment of the fire-eaters has much to recommend it. Walther offers the best biographical references in print for Miles, Keitt, and Yancey. For Tucker and Quitman, Walther does not significantly enhance what we know from the studies by Robert Brugger and Robert May; but for Wigfall, De Bow, and Rhett, he usefully updates older studies. Others can mine his impressive work in manuscript sources, thanks to careful citations and a comprehensive bibliography. In sum, students should consult this book for an introduction to nine important figures in southern history, but they must turn to more rigorous analyses by scholars like Mills Thornton, Drew Gilpin Faust, and Lacy K. Ford for insight into what radicals had in common and why they succeeded.

JOHANNA NICOL SHIELDS
University of Alabama,
Huntsville

PHILLIP THOMAS TUCKER. *The Confederacy's Fighting Chaplain: Father John B. Bannon*. Tuscaloosa: University of Alabama Press. 1992. Pp. xi, 254. \$32.95.

Father John B. Bannon was a Catholic priest in St. Louis when the Civil War broke out in 1861. Casting

his lot with the southern cause, he served as chaplain to the First Missouri Brigade in the western theater of the war until 1863 and became known as the "fighting chaplain" as a result of his actions at the Battle of Elkhorn Tavern, where he joined the crew of a rebel battery at a critical moment in the fight. After the fall of Vicksburg, Jefferson Davis and Judah P. Benjamin sent Bannon to Ireland in the hope that he might be influential in stopping or slowing the flood of Irish immigrants to America (who more often than not ended up wearing Union blue). While in Europe Bannon also attempted to secure diplomatic recognition for the Confederacy from the Vatican.

Bannon is an interesting character, but Phillip Thomas Tucker faced some difficult obstacles in trying to chronicle Bannon's role in the war. Of necessity, Tucker bases much of his account on Bannon's own ten-page memoir, "Experiences of a Confederate Chaplain," and on a diary in which Bannon wrote only the most cryptic sentence fragments. Much of Tucker's narrative, therefore, is an effort to fill in the context for these brief diary entries. Tucker does a commendable job piecing together Bannon's movements, but often he can do little more than place Bannon at the scene of events. The result is a narrative account of the First Missouri Brigade, with Tucker noting those events where Bannon was "most likely" a witness or participant. Unfortunately, Tucker's book came out too soon to take advantage of Phil Gottschalk's new comprehensive history of the First Missouri Brigade, *In Deadly Earnest* (1991).

Tucker's account takes on new life in the last two chapters, where he illuminates Bannon's role as a Confederate diplomat. Indeed, the success of these chapters makes one wish that Tucker had developed more fully the tantalizing themes broached early in the book about Irish attitudes toward the American Union and rebellion. Eager to obtain papal approval for the Confederacy, Bannon made two trips to Rome to seek an audience with the pope, and while in Ireland he managed to convince a number of parish priests to warn their parishioners against emigration to America. When the war ended, Bannon stayed in Ireland, where he died in 1913.

Tucker has combed an astonishing number of archival sources in preparing this work, and he has told us as much as we are likely to know about Father Bannon, but the archival support is so weak that only occasionally—where Tucker has something more than Bannon's diary to go on—does he manage to show us a three-dimensional figure acting in a meaningful historical context.

CRAIG L. SYMONDS
U.S. Naval Academy

GARRY WILLS. *Lincoln at Gettysburg: The Words that Remade America*. New York: Simon and Schuster. 1992. Pp. 317. \$23.00.

This book is more than a study of the Gettysburg Address. Garry Wills uses the address to make a larger argument about Abraham Lincoln's political thought and the American political regime.

One way to approach this book is historiographically. Wills's Lincoln is not the capitalist or entrepreneurial self-made Lincoln envisioned by scholars such as Gabor Boritt, Eric Foner, or Richard Hofstadter. Nor is Wills's Lincoln the tortured personality pictured by 1970s scholars influenced by psychoanalysis, scholars like George Forgie, Dwight Anderson, and Charles Strozier. Rather, Wills sees Lincoln as a committed nationalist, a sober leader trying to work out the implications of the phrase "all men are created equal." The Gettysburg Address was part of Lincoln's effort to make clear the priority of the nation over the states. Lincoln, Wills argues, really did want to direct "a new birth of freedom." And, according to the author, Lincoln succeeded. After Lincoln, the nation could think about itself differently. Against varied proponents of states' rights theory, Lincoln helped enshrine a lasting and powerful claim that the constitution was a deeply nationalistic document.

Unlike the psychoanalytic historians, Wills does not see Lincoln (the son) at war with the Founding Fathers. Instead, Wills paints Lincoln as attempting to fulfill the promise of the Founders. Wills is especially good discussing how Lincoln used the Declaration of Independence as a point of reference but injected his own nineteenth-century evolutionary thinking to explain how the Declaration's ideal should be gradually realized over time.

Instead of a Lincoln driven by his demons, a theme in both Hofstadter's *American Political Tradition and the Men Who Made It* (1948) and the psychoanalytic biographies, Wills pictures Lincoln as a keen intellect. He was not a mystic, Wills claims, but rather a thoughtful idealist who still respected the constraints of his time.

This portrait of an intellectually incisive nationalist, of an idealist hoping to secure the promise of American life, sounds very much like the Progressive Era's Lincoln. It is almost exactly the Lincoln of Herbert Croly (Lincoln, even more than Teddy Roosevelt, was the hero of *The Promise of American Life* [1909]) or of Lord Charnwood's important 1916 biography. (It also sounds much like Harry Jaffa's Lincoln.) Wills resurrects an older Lincoln, albeit with a frank discussion of Lincoln's racism that did not appear in the earlier versions.

Wills clearly explicates Lincoln's intellectual links to Whig nationalists like Daniel Webster, Joseph Story, and Henry Clay. And he breaks new ground when tying the ceremony at Gettysburg to the nineteenth-century movement for rural cemeteries. The same can be said of Wills's discussion of the theme of rebirth through death, so important in the Gettysburg Address. Wills convincingly traces this back to

Greek funereal oratory and the Greek revival of the nineteenth century.

At times Wills is too facile, pressing half-truths further than he should. Yes, Lincoln loved rationality, but it is still very odd to claim that the person who wrote the Gettysburg Address or Lincoln's First Inaugural was principally attracted by "the logical side of language" (p. 163). And yes, if Lincoln was not simply a mystic, Wills underestimates the organic, religious, mystical dimension in Lincoln's Civil War rhetoric. What about those "mystic chords of memory"? And yes, Lincoln certainly believed, by the time of the war, in a spiritual, transcendental realm, but Wills mistakenly makes Lincoln sound like he was influenced by New England transcendentalist intellectuals. There is simply no good evidence presented for trying to make Lincoln sound so close to Theodore Parker.

Despite reservations about details, this book is a real achievement. It is a testament to Wills's independence that he can resurrect an older portrait of Lincoln; it is a testament to his intellect that he can so convincingly put this portrait in a contemporary frame. And like all of Wills's work, it is so well written that it makes me jealous.

KENNETH CMIEL
University of Iowa

JAMES TALMADGE MOORE. *Through Fire and Flood: The Catholic Church in Frontier Texas, 1836-1900*. (The Centennial Series of the Association of Former Students, Texas A&M University, number 42.) College Station: Texas A&M University Press. 1992. Pp. xiii, 284. \$39.50.

Over the past twenty years American Catholic historiography has undergone a sea change. Hagiographic treatments of saintly prelates and priests and worshipful accounts of pious and courageous male and female religious, which once had been the staple of Catholic historians eager to promote the faith and self-consciousness of Catholicism's secondary status in academic circles, have now been banished to the back shelves as a new generation of scholars has brought the history of the church and its people into the mainstream, even the forefront of the "new religious history" in America. Led by Jay Dolan and spurred by Vatican II with its emphasis on lay concerns, scholars have shifted the focus from the pulpit to the pews, from the chancery to the parish. Questions of lay initiative, ethnic identities, and the devotional and social lives of congregants now dominate the stage. In much recent writing the church as an institution serves as a backdrop rather than as the sole subject worthy of attention. At the same time, modern students of American Catholic history have begun to essay the effects of local and regional settings and

cultures on Catholic identities. The history of Irish Catholics in New York or Chicago no longer stands for all American Catholic experience.

In light of these and other profound shifts in approaching the history of Catholics and their church, it is surprising to encounter James Talmadge Moore's unabashed reversion to old-school habits of viewing the church almost wholly through the eyes of bishops and priests and casting the story in heroic terms, with all the world arrayed against the good intent and deeds of Catholic fathers. Moore's book is less a history than a chronicle, almost a day-by-day rendering of church building in frontier Texas. Designed to be the first of a projected two-volume work that will carry the story through the twentieth century, Moore's Texas church is narrowly conceived. He sets the story in the context of the changing political realities of Texas, with church growth moving in tandem with Texas's break from Mexico and its increasing incorporation into American politics and culture. He notes the varieties of Texas Catholicism that came with the annexation of a native Mexican Catholic population, European immigration (Irish, Germans, Poles, and Czechs, among others), and native-born American in-migration, and he relates the difficulties of establishing uniform Catholic instruction and pastoral oversight in so varied, and sometimes so hostile, a physical environment as Texas. But in the end, Moore's chronicle fails to inspire or inform. The one-dimensional depictions of clergy all engaged in selfless church planting flattens the narrative and vitiates the sense of urgency, the personal and cultural conflicts, and the insecurity that wracked the Texas church and Catholics. Lacking any comparative framework, Moore finds no way to measure the Texas experience with that in other frontier or southern settings. Most disappointing, Moore's overreliance on, and too often literal reading of, the letters and records of bishops and priests portrays the laity as objects to be acted on, seemingly with no lives or thoughts of their own. What it meant to be Catholic in Texas is nowhere evident amid all the fire and flood of frontier Texas.

Moore enlivens his account with many nuggets on migration and the patterns of church building and frontier missions that will repay efforts to dig them out, but the larger chronicle provides only the most rudimentary materials for understanding the evolving character and history of the church in Texas. Students of the new religious history and Catholic historians imbued with post-Vatican II sensibilities will find much work to do in discovering and recovering the history of the Catholic church in Texas, one that includes people and beliefs and realizes that the church meant (and means) more than bricks and mortar.

RANDALL M. MILLER
Saint Joseph's University

B. CARMON HARDY. *Solemn Covenant: The Mormon Polygamous Passage*. Urbana: University of Illinois Press. 1992. Pp. xxiv, 445. \$34.95.

The Progressive Era in American history has received much commentary, but seldom so adroitly delivered, so gripping in topic, so heartrending in description of the human experience, so unexpected to the changes in the intermountain West, nor so compelling in its social arguments than in B. Carmon Hardy's book. Even the question most commonly assumed regarding the Mormons—how did they adopt and practice polygamy?—has received little press and less publication. Now with this book Hardy asks and answers the more pertinent question nobody seemed to ask, which is, having embraced polygamy for almost a century, how did they become monogamous? And what price did they pay for monogamy?

Hardy places the Mormons' nineteenth-century commitment to "the principle" in its historical context, coming at the end of a previous century's European debate so widespread that advocacy of polygamy appeared epidemic. Coupled with utopian movements, frontier sexual laxity, the assumed proclivities of men, and religious enthusiasm, polygamy provided the Mormons with their most distinctive identification code during the turbulent years from 1842 in Illinois to the end of World War II. After the Mormons moved west in 1847, and particularly after the public acknowledgment of the formal inclusion of polygamy into Latter-day Saint theology in 1852, polygamy marked the western Mormon movement until the *quid pro quo* of Utah statehood saw it formally abolished by the church in 1890. Or was it? It is this period from 1890 to 1920, illuminated by a scholar's keen eye and social sensitivity, that forms the subject matter of this remarkable book. Pressed by the exigencies of survival, Mormon leaders succumbed to the punitive power of a Congress determined to uphold the traditional monogamous family by accepting the "1890 Manifesto" prohibiting its continued practice. But rather than abandon plural marriage altogether, they continued to authorize and perform polygamous marriages in the face of repeated denials of so doing. This left in limbo those families who found it difficult enough to maintain their polygamous life style in the face of stiff societal opposition, but extremely painful to do so when the practice was expediently abandoned by their church as an embarrassment. By 1920 the Mormons emerged from the ordeal a monogamous people, quintessentially family oriented in American society, but leaving remnants of polygamous belief that are still present in scattered enclaves throughout the West. Hardy documents this change, but he accomplishes more than mere documentation. He builds the connecting links to an American society the Mormons naively believed they left behind, and he puts to lie those frequent images of husbands surrounded by several shrill wives that caricatured the

Mormon image in the public eye. By the end of his unique final chapter, titled "Lying for the Lord: An Essay," the moral dilemma of a people committed to belief in modern-day revelation who discover they are at odds with equally powerful requirements to obey national legislation resulted in what the author terms "inventive capacity" for use of mistruth as a device to both maintain the practice of clandestine extra-legal plural marriage and simultaneously to obliterate it from the modern-day Mormon experience. Those who believe it could not happen both ways should read this book. Written with extraordinary grace and clarity and meticulously researched, the book's conclusions and descriptions must be included in every future analysis of the American West's adaptation to the twentieth century. To do less ignores the experience of one of its most populous groups.

VALEEN TIPPETTS AVERY
Northern Arizona University

ROBERT H. CRAIG. *Religion and Radical Politics: An Alternative Christian Tradition in the United States*. Philadelphia: Temple University Press. 1992. Pp. vii, 307. \$34.95.

Religion and politics have had a long, diverse, and controversial relationship over the course of U.S. history since the Civil War. Robert H. Craig undertakes to describe part of that story, focusing specifically on what he calls the radical Christian tradition. That itself is potentially an extensive journey, but Craig concentrates on the radical tradition defined mostly as socialist, and Christian radicals defined mostly as Christian socialists. For him, radical also includes Christians believing in racial and gender equality, but Christian radicals in his highly sympathetic account were, above all, fighters against capitalism and America as a capitalist nation.

This agenda remains large, which is why Craig wisely is selective in his examples of the radical Christian tradition. He considers five main moments in his story. Each is a good choice and an important one, and the entire book is often informative. Craig examines labor radicalism expressed in the Christian Labor Union and then by the Knights of Labor in the second half of the nineteenth century; women radicals exemplified by the very different witness of Frances Willard and Mother Jones; the role of Christian Socialists in the early twentieth century, including a fascinating and enlightening discussion of several voices of Christian socialism in the African-American community; selected radical Christian ministers from the South in the 1930s and 1940s; and, finally, the postwar radical Christians Dorothy Day, A. J. Muste, Harry Ward, and Reinhold Niebuhr, a chapter that makes explicit Craig's contemporary political perspectives.

Taken together, this book is an intellectual history of Christian radicalism of a largely socialist/commu-

nitarian variety. Its spirit is deeply influenced by Craig's affinity for Christ as a radical, for Marx, and for the American Christian radical tradition he has studied in detail. The overall approach is one that Craig characterizes as "a collection of stories." The focus is largely on particular individuals, their lives, and their ideas. Much research has gone into each portrait and group of portraits, research grounded in primary sources and secondary works. This is also very much an interpretive study, both in Craig's argument that there is a distinct tradition of Christian social and political thought in our history and through his own personal perspectives throughout this work.

What this work does not pretend to be is a project devoted to political actions or events, nor to the actual influence of this tradition beyond specific persons and places discussed. Moreover, Craig makes clear that he wishes his study to be different from what he considers conventional U.S. religious history. He regrets that so much of that history has focused on "institutional development, disembodied theological debates, and the experience of transplanted European immigrants" (p. 229) when there is so much else to be told about Christianity in the United States.

Although he is not especially intrigued by theology or theologians, Craig is quite clear, as he must be, about what he and his favored subjects consider Christianity to be. In a phrase, its essence appears to be "fidelity to the gospel as good news to the poor" (p. 230). It is in the context of that understanding that socialism emerged again and again among Craig's Christian radicals.

This discussion, however, cannot explain all that this work offers to those seeking knowledge of radical Christianity in the United States. The strength of this book is its real substance.

ROBERT BOOTH FOWLER
University of Wisconsin,
Madison

FRED W. PETERSON. *Homes in the Heartland: Balloon Frame Farmhouses of the Upper Midwest, 1850-1920*. (Rural America.) Lawrence: University Press of Kansas. 1992. Pp. xi, 296. \$35.00.

Fred W. Peterson presents us with a compelling field-based study of vernacular architecture in the American upper Midwest. Beginning with the advent of balloon-frame construction—an efficient method for erecting buildings using dimension-sawn lumber nailed together—Peterson organizes his study of farmhouses into categories or types of dwellings based on characteristics of massing and plan. Each type is then associated with a discussion of settlement, style, modernization, aesthetics, class, or other historical processes. This simple formula represents a complex interpretive undertaking that Peterson pursues

with dedication overall and with real flair in several key passages.

This book represents the growing trend in material culture and vernacular architecture studies to address the sorts of questions long associated with social history. Central to the success of this enterprise is the ability to read objects, in this case buildings, as historical evidence. Two questions remain: what are the "rules" for reading this evidence, and does material culture provide us with any new insights? Peterson addresses both these undercurrents. As Peterson demonstrates, the rules for reading architecture as historical evidence begin with the close analysis of the objects themselves. Through a detailed investigation of balloon-frame construction techniques, Peterson identifies the possibilities and limitations that informed the work of local builders. The architectural expression of domestic and social ambition can be no more elaborate than the means available for realizing its achievement. What Peterson finds in the easily built balloon-frame houses is a discernible rise in levels of accessibility (who can build) and expression (what can be built). For Peterson, the net result of this rise is what he questionably describes as a democratization of architecture.

The rules of material evidence extend beyond matters of technology into the exploration of farmhouse types (what people built). Carefully recording the floor plans and massing of farmhouses throughout the upper Midwest, Peterson organizes these plans into twelve categories based on physical attributes. Here Peterson breaks new methodological ground by using the idea of architectural type as the means to engage broader thematic concerns associating his types with specific aspects of the region's settlement and social history. One of the most successful discussions in this regard begins with the processes of "consolidation and standardization" and successfully expands to include the relationship between farmhouses and the technology and business of farming.

This book is not without its flaws. Looking at farmhouses alone provides little sense of the place of these dwellings in a larger landscape of agricultural buildings and the architecture of rural towns and villages. Did the same building technology, for example, engender a similar range of types in barn design, and did barn design parallels present a complement or paradox to types of houses? In the same vein and somewhat more troubling is the lack of an overall sense of history. Peterson talks about houses and history, but the relationship between building form and change over time is never as explicit as it should be. Students of historic landscapes typically find a confusion of architectural expression at any given moment in time. The fact that Peterson was able to document so many different types of houses still standing and often with apparently overlapping dates of construction speaks to the issue of tensions and contested economic and social relationships in the

rural world. Also missing from Peterson's discussion are sustained interpretations of what house plans mean in terms of how houses are occupied and experienced in the contexts of ethnicity, gender, economic and productive relationships, and class. While Peterson speaks to some of these interpretive problems within his individual farmhouse types, he addresses neither the relationships between types nor the visual and symbolic relationships between types in the larger countryside.

Peterson does give us the first in-depth examination of the architectural evidence of the upper Midwest. Where other scholars have wrestled with the evidence of colonial and early republican architecture in the older settled areas of the east, Peterson convincingly adapts their techniques to the little understood landscapes of America's heartland. Most importantly, Peterson demonstrates the potential and use of architecture for the purposes of writing social history. Peterson vividly reveals that historians would do well to expand their evidential horizons to include what people made as well as the little they bothered to write down.

BERNARD L. HERMAN
University of Delaware

DAVID ALAN JOHNSON. *Founding the Far West: California, Oregon, and Nevada, 1840-1890*. Berkeley and Los Angeles: University of California Press. 1992. Pp. xiv, 474. \$35.00.

This detailed account of the first half-century of settlement in the Far West, as David Alan Johnson calls the geographical setting for his study, is distinctive in many ways. For example, in the author's selection of states, he chooses Oregon from the Pacific Northwest (thus excluding neighboring Washington), Nevada from the Great Basin, and California from across the Sierras, the latter state more often than not regarded as part of the American Southwest. Even more unusual, he relates the early histories of these three states through the lives and activities of the people responsible for the founding of the Far West, to use the book's title; these are the pioneers who comprised the "charter groups" that drew up the constitutions for each of the states. Moreover, in his documentation of sources used in the study, he relies solely on notes, omitting the more traditional bibliography or bibliographical essay.

The results of Johnson's innovative approach, however, are gratifying. Incorporating California, Oregon, and Nevada in a history of the Far West makes good sense, given Johnson's historical focus. A preponderance of early settlers from the expansionist United States came by way of the Oregon Trail; those seeking gold went to California, those seeking farmsteads went to Oregon. To use an old yarn that has delighted Oregonians: "At Pacific Springs a pile of gold-bearing quartz marked the road to California;

the other road had a sign bearing the words 'to Oregon.' Those who could read took the trail to Oregon" (p. 8). Finally, in the late 1850s, with the gold and silver discoveries in Nevada symbolized by the fabled Comstock Lode, "Old Californians" crossed the rugged Sierras to open up a new commonwealth in the arid Great Basin.

But the emphasis in this political, almost ideological, study are the careers and contributions of the region's constitution makers. These people collectively, as delegates to each state's constitutional convention, framed a document that reflected the ideological views of the territory's hopeful leadership, if not its majority. California delegates, for instance, hammered out a basic instrument at Monterey in 1849 which was much in harmony with their belief that an environment where individualism was allowed to thrive was most conducive to growth and prosperity. Oregon delegates, formulating their constitution at Salem in 1857, advanced a classical republican approach where individualism was often subordinated to the common good. The result was a "liberal commonwealth" based on political concepts and ideas that dated back to the eighteenth century. Nevada delegates, employing a model of California's constitution, tempered the individualistic tone of their new basic law, which was crafted in Carson City in 1864, to make it more palatable with the corporate capitalism that had transformed mining activities in Nevada into something quite different from the California experience.

This study should command wide notice among students of Western history not only because of its distinctive focus and organization but also because of its substantial contribution to our knowledge of this region. Territorial historians particularly will appreciate the way Johnson traces the evolution of political thought for each of the three Far Western states from those constitutions in the East and Midwest, which provided useful models. The extensive biographical information about the constitutional delegates will prove especially enlightening to the people of this region; indeed, information lacking in the text can often be found in one of the book's helpful appendixes. The lack of a bibliography is more than compensated for by notes that are extensive, bibliographical, and reflect the impressive research that has gone into the completion of this study.

ROBERT W. LARSON
Aurora, Colorado

WILLIAM B. FRIEDRICKS. *Henry E. Huntington and the Creation of Southern California*. (Historical Perspectives on Business Enterprise Series.) Columbus: Ohio State University Press. 1992. Pp. 229. \$35.00.

Henry E. Huntington was a turn-of-the-century railroad tycoon who is best known for founding the Henry E. Huntington Library and Art Gallery in San

Marino, California. Located on the Huntington estate, the magnificent library is set in a vast formal garden where scholars and tourists stroll and think about some of the material advantages that come with great wealth. Those who would like to know some of the details of how Huntington acquired his assets may turn to the slim volume by William B. Friedrichs.

The general outlines of the Huntington story are well known. He was the favorite nephew of childless Collis P. Huntington, the financial wizard of the Big Four who built the Central Pacific and Southern Pacific railroads. Collis trained Henry in the railroad business and left him one-third of his sizeable fortune. E. A. Harriman and others blocked Henry's attempt to replace Collis as the president of the Southern Pacific, so he turned his attention to southern California. Armed with experience and money, Huntington developed an elaborate network of inter-urban railroads, real estate holdings, and a power company. The railroads were not very profitable, but Huntington used them to increase the value of his real estate holdings by extending lines to the suburban properties that he was developing. Like most capitalist entrepreneurs, Huntington hated organized labor and used every means to destroy unions and maintain an open shop. Finally, Huntington got out of his various enterprises and gave his energy to building the library, art gallery, and gardens that bear his name along with many other southern California places, including Huntington Park and Huntington Beach.

Friedrichs has written primarily about Huntington's business career. He argues that Huntington was exceptionally far-sighted and single-minded in devising an overall plan for the development of southern California. To me, Huntington seems much like his peers among successful industrialists and businessmen. They also had good ideas and stuck with them because they were profitable. They also hated labor unions. Many of them turned to philanthropy after their fortunes were made. This is not to deny Huntington's accomplishments or his instrumentality in shaping the Los Angeles region, but Huntington flourished in a community of businessmen and promoters who appreciated his vision, even though they lacked his purse and special expertise. Friedrichs adds many important new details of Huntington's business dealings, but some readers will want to know more about his personal life and the social and cultural milieu that made his accomplishments possible. Nevertheless, the book will be of interest to historians of Los Angeles, business, railroads, and urban development.

ALBERT L. HURTADO
Arizona State University

JOHN H. ELLIS. *Yellow Fever and Public Health in the New South*. Lexington: University Press of Kentucky. 1992. Pp. xii, 233. \$28.00.

Throughout the late summer and fall of 1878, a disastrous epidemic of yellow fever swept through the South and up the Mississippi Valley, striking terror into 200 communities from New Orleans to Gallipolis, Ohio. In New Orleans, Memphis, and other valley communities yellow fever claimed thousands of victims and thousands of others abandoned their communities to escape from the spread of this dread disease. In this brief volume, John H. Ellis traces the responses to the illness in three communities (New Orleans, Memphis, and Atlanta), seeking to explain the differing experiences and responses to the illness.

The book begins with a review of the structural realities of life in the three communities. Rapid population growth combined with a virtual absence of sewerage, pure water, and decent housing created the preconditions for the spread of infectious diseases in general. The physical site of New Orleans and Memphis in particular made the communities especially prone to the spread of infectious diseases. Built on bayous and marshes, riven by class and racial divisions, and with little capital to invest in expensive sanitation systems in the years following the Civil War, New Orleans and Memphis became known as pestilential, as "necropolitan" areas.

Ellis ascribes the communities' inability to respond quickly to onslaughts of epidemic disease to various political and social divisions that often paralyzed the cities' governments. Divisions among those who feared the economic impact on the commercial economy led to temporary and inadequate systems of inspection and quarantine. He also traces some of the racial stereotyping that led to inaction. Since African Americans were often the poorest and most susceptible to illness, wealthier white communities often took solace in believing that their former slaves were particularly culpable in bringing the disease on themselves.

By the early 1900s, yellow fever largely ceased to take its ferocious toll. In part, the end of the yellow fever epidemics was due to sanitary reforms that were hesitantly adopted by the cities' governments. Like other large cities, sewer lines and pure water systems were introduced, and permanent boards of health replaced the emergency committees that previously were assembled during the crisis months of an epidemic. But other demographic, economic, and social factors were also transforming these southern cities, thereby destroying the environments and the circumstances that first allowed yellow fever to become epidemic.

Despite some admirable research and an often engaging narrative, there are important limitations to Ellis's analysis of the events. For example, he has interesting information on the importance of race and racial division in understanding cities' activities in controlling disease and improving infrastructure. But the examination of the interaction of political responses, social realities, and racism rarely goes very far. Also, the responses to the disease do not seem

especially unique to the "New South," thereby raising questions about what is emblematic of the region during this important period. Nevertheless, Ellis does make it clear that we, as a society, create the conditions within which diseases thrive.

DAVID ROSNER

Baruch College

Graduate School and University Center,
City University of New York

RONALD RAINGER. *An Agenda for Antiquity: Henry Fairfield Osborn and Vertebrate Paleontology at the American Museum of Natural History, 1890–1935*. (History of American Science and Technology Series.) Tuscaloosa: University of Alabama Press. 1991. Pp. xiii, 360. \$37.95.

Unscrupulous and egotistical, hated or ridiculed by many of his colleagues, Henry Fairfield Osborn muscled his way into the position of power he occupied in American paleontology for over forty years. Through popular writings and museum displays, his opinions were absorbed into popular culture. We do not have to like him, but if we are concerned with evolutionary biology, we must understand him, and Ronald Rainger's scholarship now makes this possible.

At the American Museum of Natural History (AMNH) in New York between 1891 and 1933, first as curator of the Department of Vertebrate Paleontology and later as president of the museum, Osborn vigorously pursued his ambitions, the "agenda" of Rainger's title: to make the AMNH a world leader in the study of fossils, to put evolutionary theory at the center of paleontology, to combat the rise of materialistic science, to preserve traditional social and religious values, and to prevent the decline of humanity. To a remarkable degree these items were inseparable for Osborn, and Rainger leads us to understand the connections between his active involvement with the eugenics movement, his belief that living humans belonged to several species, his perception of divinity expressing itself in the cusps of fossil teeth, his interest in biogeography, and his opposition to the new science of genetics. For Osborn, evolution was an inspiring vision of sacred Nature, operating in accordance with laws accessible only to men with the courage to embark on far-flung collecting expeditions, men with the imagination to wrest answers from silent rocks as no prideful experimental scientist could do. It did not matter, as Rainger reveals, that Osborn's own field work consisted mostly of tours of inspection overseeing the discoveries of employees, nor that his own imagination was so uncritical that he could insist that the ancestor of human beings had no demeaning similarity to any ape. Rejecting both neo-Lamarckism and neo-Darwinism, Osborn invented his own vacuous substitute, "aristogenesis," which although convincing to no other scientist, was broadcast through popular books like *Men of the Old Stone*

Age and From the Greeks to Darwin. In the halls of his great museum, exhibits like the famous horse series, Charles R. Knight's murals, and Adam Hermann's wonderful dinosaurs embodied Osborn's convictions about evolution.

Those who wish to prove that science's claims to objectivity are groundless can pull out plum after plum from Osborn's basket of ugly prejudices, but Rainger, while reminding us several times how self-interested and ill-founded Osborn's views were, refuses to let us dismiss him. After all, Osborn did translate his conviction that paleontology ought to address evolutionary questions into a substantial and continuing research enterprise. Rainger traces the work of two of Osborn's students, William Diller Matthew and William King Gregory, establishing in detail how greatly their questions and methods were formed by Osborn's interests. We get another view of his leadership when Rainger proves that even when associates explicitly contradicted his beliefs, Osborn encouraged and supported them. These students in turn, most notably G. G. Simpson, integrated into paleontology the genetics Osborn had rejected. Those who still believe that science is a self-correcting process can find here the tale of a discipline transcending the demerits of individuals.

MARY P. WINSOR
University of Toronto

MICHAEL H. GORN. *The Universal Man: Theodore von Kármán's Life in Aeronautics.* (Smithsonian History of Aviation Series.) Washington, D.C.: Smithsonian Institution. 1992. Pp. xii, 202. \$24.95.

Theodore von Kármán led a life of historic substance and rich style. Michael H. Gorn's short, informative, appreciative biography covers both. It captures the style better than the substance.

A student of the famous aerodynamicist Ludwig von Prandtl, Kármán eclipsed his mentor, according to Gorn, in a career that spanned six decades, three continents, and easily half a dozen different fields and disciplines. Virtually all of his work was characterized by application of mathematics and science to engineering problems, many of them in aeronautics and astronautics. He is perhaps best known for his mathematical analysis of the eddies that form behind solid surfaces moving through a fluid stream, called in his honor Kármán Vortex Streets. But he made similarly important contributions in the areas of strength of materials, aircraft structures, high-speed flight, and wind erosion of soil (which Gorn does not cover). He elevated the Aeronautical Institute at Aachen, Germany, to world stature in the 1920s, and then moved to California and did the same for the Guggenheim Aeronautical Laboratory at the California Institute of Technology. He encouraged students at Cal Tech to pursue their interests in rocket research before World War II and joined them in founding the

Aerojet General Corporation to exploit their findings. At the end of World War II he directed a massive study for the U.S. Army Air Forces on the future of air power; this became the basis of air force research and development policy well into the Cold War. In his declining years, he left his adopted country to return to Europe, taking up direction of the NATO Advisory Group for Aeronautical Research and Development in Paris.

This substance, however, is inseparable from Kármán's style, to which Gorn pays even greater attention. Hungarian by birth, Kármán carried through life a cosmopolitan appetite for food and drink, a weakness for three-Manhattan lunches, and a sheer delight in festive parties that mixed work and revelry late into the night. He was by all reports a raconteur of the first order, equipped with anecdotes for any occasion and a love of social intercourse that infected whatever group he graced. Not surprisingly, this combination of scientific power and social conviviality made him a great teacher; his students admired him and remained loyal to him throughout his career.

But something is missing. Behind Gorn's warm and appreciative portrait lurk unanswered questions. Kármán seems to have relished being the center of attention and to have had a real flair for self-promotion. He never married and lived much of his adult life with his mother and his sister, to whom he had a powerful attachment. Just as he seems never to have escaped the centripetal force of his own family, so too he seems to have kept his students locked in orbits that circled him and reflected his light. Hidden behind the bright glow of this amazing life and its colorful style are darker forces that Gorn does not explore.

ALEX ROLAND
Duke University

NICHOLAS FOX WEBER. *Patron Saints: Five Rebels Who Opened America to a New Art, 1928-1943.* New York: Alfred A. Knopf. 1992. Pp. xi, 398. \$30.00.

Four men and one woman, Nicholas Fox Weber contends in his subtitle, "opened America to a new art." The individuals he nominates for patron sainthood deserve appreciation for their curiosity and dedication, their energy and generosity, but their grouping appears forced and incomplete.

Children of wealth and cultivation, well educated and widely traveled, they marched lustily into the perennial American cultural war against philistines. That each heard a different drummer complicates this book: five busy people, each charging off in several directions, their lives intersecting only intermittently.

Most absorbing (because it is based on extended conversations) is Weber's portrait of Edward M. M. Warburg. Weber touchingly describes how the witty, light-hearted, rich boy fell into modern art at Har-

ward and fulfilled his heart's desire: to belong to "a gang." Warburg and his family prodigally funded the new art gang's many capers, until Nazi persecution of Jews diverted the money to saving lives.

The most accomplished in the group was Lincoln Kirstein, who would devote his long life to George Balanchine and the New York City Ballet. Weber portrays Kirstein's passion for art, but W. MacNeil Lowry's two extended "Conversations," published in *The New Yorker* (Dec. 15, 22, 1986), far better convey Kirstein's prickly intelligence, his acerbic caprice, and his Mosaic commandments on aesthetic matters.

James Thrall Soby and A. Everett (Chick) Austin appear as the twin *enfants terribles* of Hartford, Connecticut. Soby, the eclectic art collector, and Austin, the playful director of the Wadsworth Atheneum, savored shocking staid Connecticut insurance executives out of their actuarial tables. They also threw great parties. But the real Soby and Austin remain muffled by Weber's reticence. We never do find out, for example, why Austin in 1941 abruptly took a one-year leave from the Hartford Atheneum, quit the director's job the following year, and in 1945 moved to Hollywood. Nor do we learn what he died of, at age fifty-six.

Least convincing is the sketch of Agnes Mongan, who devoted her life to Renaissance drawing and to the Fogg Museum of Art at Harvard. Her obsequious letters to Paul Sachs, her boss at Harvard, and to Bernard Berenson offer heartbreaking evidence of many women scholars' sorry status before World War II. No evidence, however, supports Weber's implication that Mongan was a major figure in introducing "new art."

Nor does Weber convincingly supply any reasons why these five deserve to be canonized, as distinguished from the many others—especially the enthusiasts at the Museum of Modern Art (MOMA)—who brought new art before Americans. Indeed, Weber's determination to downplay the MOMA leads to some striking omissions. He ardently describes lectures by Gertrude Stein and Le Corbusier at the Wadsworth Atheneum, for example, but fails to mention that these avatars of modernism were imported by the MOMA, whence they were booked for appearances elsewhere.

This book is most effective as a nostalgic glimpse of high jinks among the affluent avant-garde. Weber lingers for five pages on the vogueish minutiae of the Venetian fete Austin organized on arriving at the Atheneum in 1928 (pp. 136–40). He lavishes some twenty pages on the 1934 Atheneum production of the Gertrude Stein–Virgil Thompson opera, *Four Saints in Three Acts*. He devotes ten pages to describing who attended the First Hartford Festival, and what they wore to this week-long revel of experimental music, film, dance, and art, culminating in the splendiferous Paper Ball, designed by Pavel Tchelitchew

(pp. 293–30). "Enormous fun to read," says the book jacket. Amen.

ALICE GOLDFARB MARQUIS
*University of California,
San Diego*

RICHARD GREENING HEWLETT. *Jessie Ball duPont*. Gainesville: University Press of Florida. 1992. Pp. xvi, 357. \$39.95.

Richard Greening Hewlett's biography of Jessie Ball duPont is primarily an account of her charitable and philanthropic activities from 1921, when she married Alfred I. duPont, until her death in 1970. Hewlett was commissioned by the Jessie Ball duPont Religious, Charitable, and Educational Fund to write the history in order to inform a new generation of trustees of the purposes and direction the founder envisioned. Fully half of the book is devoted to an explanation and description of Jessie duPont's management of the \$100 million she gave away.

The first part of the book is an engaging account of the early life of Jessie Ball duPont that reads almost like a fairy story. She was a woman of humble background from rural Virginia who taught school for a few years in California, and later she married the man of her dreams who made her very wealthy. She was born in 1884 in the tidewater area of Virginia to poor but genteel parents. She was able to earn a teaching certificate which allowed her to teach in Virginia and later in San Diego, California.

Jessie first met Alfred I. duPont when he visited her family as she was growing up. He was twenty years older than she and a member of the wealthy duPont family of Delaware. Her secret love for him flowered in 1921 when she and Alfred were married after the death of his second wife. Evidently Jessie and Alfred were devoted to each other during the sixteen years of their marriage.

When Alfred died in 1935, Jessie took over the management of his estate, which by that time was extensive. Her later life centered around the duPont estates in Delaware and in Florida, where the family owned timberlands, real estate, banks, and commercial property. The Florida interests, which virtually dominated the economy of the state, were managed by Jessie's brother, Edward Ball. Jessie's many philanthropic activities included gifts to needy college students, private universities, hospitals, community centers, churches, and historic preservation projects.

Hewlett tells an intriguing story of the interpersonal relationships among the duPonts. Years of feuding within the duPont clan had cut Alfred off from his immediate family. Jessie tried to reconcile Alfred and his sister, Marguerite, but was successful for only a short period. Marguerite saw Jessie as a money-grubbing opportunist who spent vast sums on houses and gardens instead of the poor and destitute. The task of bringing Alfred's four children by his first

wife back into a relationship with their father was even more difficult. But eventually Alfred won them over, at least to the extent that they visited him at Christmas and wrote occasional letters.

In his will Alfred made some provision for his children, but he intended the bulk of his estate to be used for charitable purposes under the direction of his wife. When it became clear that the duPont fortune was not to be shared with the family, the harmonious relationships that Jessie had nurtured broke down. Jessie continued, however, to provide for the needs of both families in crisis situations and to maintain relationships especially with the younger generation.

Jessie duPont made every attempt to carry out her husband's wishes as expressed in his will. By 1940 she had accomplished her goal, but she found the changes brought about by World War II threatened her southern conservative view of the world. She especially opposed integration of schools and withheld her money from institutions that admitted blacks. By the 1950s her giving shifted from one of charity to one of a more structured philanthropy.

Hewlett has produced a well-documented and thoroughly researched biography of Jessie Ball duPont's philanthropic works based on the papers of the foundation. He has also researched the Ball and duPont families, the history of philanthropy, and women's history. By weaving these themes together he has made clear the character and personality of a rich southern conservative woman who, while traditional in many ways, believed that women should play an active role in public affairs.

MARY MARTHA THOMAS
George Mason University

FRANK T. ADAMS. *James A. Dombrowski: An American Heretic, 1897–1983*. Foreword by ARTHUR KINOY. Knoxville: University of Tennessee Press. 1992. Pp. xiv, 377. Cloth \$49.50, paper \$22.50.

James Dombrowski, Frank T. Adams writes, "promoted the Kingdom of God with a businesslike resolve" (p. 288). Indeed, Dombrowski occupies a special place among those white southerners who, beginning in the 1930s, dedicated themselves to abolishing segregation and establishing economic, political, and racial democracy. Heresy—a word he preferred over radicalism—was Dombrowski's business. He largely worked alone, yet his associates valued his organizing, fundraising, and administrative skills. His vision of the beloved community remained inclusive, even though he was attacked by critics and allies alike. He did not seek celebrity status, but he wanted history to remember his life. Adams seeks to fulfill that desire in this competent biography, based on extensive interviews with Dombrowski as well as on his personal papers.

Dombrowski's life was defined by his service to

southern organizations. The son of a Tampa jeweler, he reached adulthood with a vague awareness of injustice and a desire to serve humanity. He graduated from Emory University in 1923 and followed his convictions by passing up a position with Coca-Cola to organize his alma mater's alumni association. Two years later he began an intellectual odyssey that among other things led to a B.D. thesis on Christian Socialism at Union Theological Seminary and a memorable stay in a North Carolina jail during a textile mill strike. Joining the staff of Tennessee's Highlander Folk School in 1933, he established its administrative foundation, helped develop its distinctive approach to labor education, engaged in several Popular Front projects, and defended the school as its controversial reputation grew.

In 1942 Dombrowski decided to press harder for racial reform by becoming executive secretary of the Southern Conference for Human Welfare (SCHW). Through SCHW Dombrowski brought middle-class liberals together with representatives of organized labor and African-American groups, but the conference suffered from various internal woes and segregationist assaults. By the time of SCHW's demise in 1948 Dombrowski had become director of its public education affiliate, the Southern Conference Educational Fund (SCEF). He turned SCEF into a highly visible organization devoted to influencing public policy on race by exposing the high costs of segregation. Anticommunist investigations, combined with the white South's massive resistance to integration, cost SCEF black and southern white support. Dombrowski persevered nonetheless, making legal history when the Supreme Court, in *Dombrowski v. Pfister* (1965), ruled that governmental prosecution of constitutionally guaranteed activities had a "chilling effect" on the exercise of First Amendment rights.

Adams's largely anecdotal account is both instructive and opaque. Simply telling the story, the author believes, not only calls attention to an influential voice outside the mainstream but also shows that there are, and ought to be, possibilities for a more just, tolerant, and democratic society. Yet stories also ask readers to suspend judgment and concentrate on their message, to accept that what has been included or left out has been determined by what the author wants to convey, and to give secondary consideration to accuracy, critical analysis, relevance, and scholarly apparatus. As a story, therefore, this sympathetic biography contributes to the growing list of studies on the "other" South.

JOHN M. GLEN
Ball State University

JON MICHAEL SPENCER. *Black Hymnody: A Hymnological History of the African-American Church*. Knoxville: University of Tennessee Press. 1992. Pp. xiii, 242. Cloth \$29.95, paper \$18.95.

This book by Jon Michael Spencer, the first comprehensive study of black American hymnody, examines more than three dozen hymnals and songbooks produced by and for African Americans in ten different religious denominations. The first such hymnal was compiled in 1801 by Richard Allen, the founder of the African Methodist Episcopal (A.M.E.) church; the latest is a black Catholic hymnal published in 1987. Allen's 1801 hymnal was a modest affair, containing the words to fifty-four hymns (without music), and since his time, African-American hymnbooks have grown steadily larger and more elaborate.

For black American denominations, publishing their own hymnals was a significant measure of their race pride. As R. H. Boyd, secretary of the National Baptist Publishing Board, stated in 1900, it was a matter of great importance that black Christians "use in their churches and Sunday schools our own song books, and read only literature manufactured and handled by ourselves" (p. 77). As Spencer makes clear, however, the fact that black churches published hymnbooks did not necessarily mean that their hymnals contained large numbers of hymns by black authors. In part, black denominations did not place a priority on including songs by black songwriters because they often felt the need, especially in the nineteenth century, to model their hymnbooks on those of other denominations to save time and money. The *National Baptist Hymnal* that Boyd published in 1903 was closely modeled after an 1883 American Baptist hymnal, one that did not contain a single hymn by a black author but instead included an excessive number of hymns written by eighteenth-century evangelicals such as Isaac Watts and Charles Wesley. Boyd's hymnal did incorporate a supplement of sixty-five songs by black authors, but even so it was clear that black hymnody was severely marginalized.

Over the past century, however, the vast and immensely creative musical output of black songwriters has been brought slowly but firmly into the center of black hymnals. Henry McNeal Turner's hymnbook, published in 1876 for his A.M.E. church, was a forerunner in this respect, including several songs by black hymn writers celebrating the emancipation of the slaves as well as at least nine hymns written by fellow A.M.E. clergy. The creative explosion represented by the arrival of black gospel music in the early twentieth century could not be ignored by the compilers of black hymnals. Spencer correctly points to the close ties between black gospel songs and the seemingly more secular blues, noting that the same songwriters (Thomas Dorsey, for instance) often created both kinds of music. In any event, nearly all black hymnals published since the 1920s have included numerous hymns by such gospel songwriters as Dorsey, C. A. Tindley, Doris Akers, and Lucie Campbell. The works of the prolific hymn writer C. P. Jones, however, founder of the Church of Christ (a Holiness church), have largely been ignored outside of the Holiness and Pentecostal denominations. Cu-

rously, it was not until the civil rights movement of the 1950s that the largest black denominations began to include black spirituals in their hymnbooks. Spencer observes that, well into the twentieth century, black Americans tended to see the singing of spirituals as evidence of a lack of education, and hence (quoting R. N. Dett) "a backward step in the progress of the race" (p. 177); but after the 1950s, these songs came to be prized, in the words of A.M.E. Zion Bishop W. J. Walls, as a "precious gift squeezed out of the toils and tears of our ancestral afflictions" (p. 42).

In the 1980s, African Americans within three predominantly white denominations, the United Methodist, Episcopalian, and Roman Catholic churches, have compiled and published their own hymnals. Spencer examines these hymnals in detail and praises each one highly. The Methodists' *Songs of Zion*, he states, is "the paradigm of black religious self-awareness, after which future black denominational hymnals must be measured" (p. 72), because of its full selection of gospel songs and spirituals and its thoughtful essays on black hymnody. All three include at least a few hymns written by Africans, whereas the hymnals published by the black denominations apparently have not yet included hymns by Africans. Looking to the future in his postscript, Spencer hopes for "replacing extant hymnody with a radically aggressive song" dedicated to removing remaining racial barriers and enhancing a corporate sense of black personhood, although he admits that this may be impractical at present.

This is a generally well-written and insightful book. The author breaks down his subject matter according to denomination; as a result, the evolution of each church's hymnody can be seen clearly, but Spencer does not draw as many cross-denominational comparisons as he might. Although this book is surely not the last word in the exploration of black hymnody, it does constitute an important advance.

STEPHEN W. ANGELL
Florida A&M University

HANS A. BAER and MERRILL SINGER. *African-American Religion in the Twentieth Century: Varieties of Protest and Accommodation*. Knoxville: University of Tennessee Press. 1992. Pp. xxiii, 265. Cloth \$32.95, paper \$14.50.

The subject of black religion in the United States is daunting in its diversity, spanning such institutions as Harlem's affluent and socially progressive Abyssinian Baptist Church, a multiplicity of ghetto storefronts, varied Islamic and Judaic sects, and rural evangelical congregations, among others. Hans A. Baer and Merrill Singer, two cultural anthropologists, aim in their study to bring clarity and order to this ever-shifting frontier of spiritual activity by identifying several basic forms of black religious expression and charting their evolution through the present. The results are mixed: they manage in a brief space to

suggest the complex and overlapping qualities of black churches, sects, and movements, but this very achievement ultimately wreaks havoc on the tidy classifying enterprise at the heart of their volume.

Grouping churches and sects based on their attitudes toward the larger society and their leanings toward either emotional expression or collective action, Baer and Singer identify four archetypes of black religious organization: mainstream, messianic-nationalist, conversionist, and thaumaturgical or spiritualist. Among institutions tending toward social activism, the mainstream denominations such as Baptists and Methodists have promoted integration into American society, whereas the messianic-nationalist sects, typically centered on charismatic leaders, have extolled black autonomy and "the unique spiritual importance of Black people." By contrast, the relatively "otherworldly and apolitical" Holiness and Pentecostal sects have looked to wrenching conversion experiences to remake people's lives, while the thaumaturgical sects have promised success through "magico-religious rituals" and "esoteric knowledge" (pp. 58–62). The authors find elements of protest and accommodation in each religious type, but they conclude that all seek "to counter . . . the twin insults of white racism and economic exploitation" (p. xxii).

A central but largely unexplored premise of this book is that blacks who accept capitalist values compromise their own interests and "unwittingly serve as hegemonic agencies of the white-dominated society" (p. 143). Baer and Singer never probe in a sustained way whether socialism in America is a realistic, let alone desirable, possibility, or how blacks—including militants—should cope during the indefinite interim. The authors might explain, for example, how the Nation of Islam, which sponsored businesses worth over \$85 million by the 1970s, could have better aided slum dwellers by spurning this exercise in "middle-class values" and "Black Puritanism." Also, could this sect have promoted socialism without embracing electoral politics, thus becoming once again an unwitting agency of accommodation?

The book is most problematic in pressing its four-cell typology, a schema repeatedly undermined by Baer and Singer's own analyses of specific ministers and movements. The authors show that Father Divine's Peace Mission, which flourished in Harlem during the Great Depression, fits all four types, an astute concession that might well be extended to many other popular black spiritual forms. The unruly complexities of black religion are evident, too, from the fact that Joseph H. Jackson, head of the largest mainstream association—presumably an activist, integrationist group—so opposed Martin Luther King, Jr.'s civil rights protests that King helped found a smaller, splinter Baptist network in 1961. The authors themselves acknowledge that "African-American religion . . . is forever resistant to facile generalization. For every conclusion, there are contradictory moments and cases" (p. 27). Their insistence none-

theless on the value of typologies even when riddled by counterexamples suggests a curious convergence of social science and faith.

ROBERT WEISBROT
Colby College

KAREN ISAKSEN LEONARD. *Making Ethnic Choices: California's Punjabi Mexican Americans*. (Asian American History and Culture Series.) Philadelphia: Temple University Press. 1992. Pp. xii, 333. \$44.95.

A book with a subtitle such as this, about people with names like María Singh and José Khan, is likely to provoke confusion, disbelief, and some mirth. Karen Isaksen Leonard's study examines this unusual combination of ethnic backgrounds and helps broaden our understanding of ethnicity. Her blending of ethnography, social history, and jargon-free, graceful prose constitutes one of the most stimulating and pleasurable studies of ethnicity I have ever read. The book should win a prize of some kind.

The outline of the group's story is little known but rather prosaic. Between about 1900 and 1915 some 7,000 men from the Punjab region in India came to work on the farms of California's Central and Imperial valleys. Most came as bachelors who planned to return, but, as was true of other immigrants, many remained. Often taking Mexican wives, they created about 2,000 communities. Leonard focuses on those who settled in the Imperial Valley, near San Diego. These settlers and their descendants remained in agriculture, and today comprise some of the "Other Asians" that official statistics and sociologists refer to after mentioning Chinese, Japanese, and Filipino people in California.

Their lives and histories abounded in contradictions. Called Hindus by Californians (they referred to themselves the same way), almost all were Sikhs and Muslims rather than Hindus. They were barred from becoming citizens and, after 1923, from owning land because they were "Asians," and objections to their racial designation provoked the reply that they may be Caucasians but were not "white." Originally regarded as the most undesirable of all Asian immigrants, within a decade or so of arrival many had become farmers; when California prohibited their owning land, sympathetic whites agreed to hold the land in their name.

The central theme of the study is how, through individual and collective choices, these "Hindus" invented and reinvented their ethnic identity. The pioneers married Mexicans (antimiscegenation laws prevented them from marrying whites) and strove for good relations with their neighbors, but they insisted on Punjabi identity in such areas as food taboos (Muslims refused pork), burial customs (Sikhs insisted on being cremated), and stress on hard work and land ownership. Finally, when in 1946 California law permitted them to become citizens, they felt free

to assert their Punjab identity more strongly. Their wives became cut off from their countrymen and bonded to each other; but culturally they remained Mexican. The children, although raised Mexican by their mothers, rejected Mexicans and identified strongly as Hindus, largely because Hindus were better off. Sadly, when the immigration law was amended in 1965 and thousands of Indians flocked to California, they were rudely rebuffed by "true Indians" and in confusion and anger forged a new identity as East Indian Americans.

These remarks can only hint at the many fascinating and touching aspects of this study. Based on extensive interviews and a nice blend of documents—marriage certificates, probate records, birth and death notices, as well as good maps and wonderful photographs—the book contains especially notable treatments of courtship and marriage, family and domestic relations, relations with Anglos and Mexicans, self-image, cultural retentions and cultural blendings, changes in religious behavior, and the changing form and content of ethnic identity. One quibble would be a lack of sufficient attention to overall social structure, landholding patterns, and the economics of farm operations.

LAURENCE A. GLASCO
University of Pittsburgh

LEO P. RIBUFFO. *Right Center Left: Essays in American History*. New Brunswick, N.J.: Rutgers University Press. 1992. Pp. 298. Cloth \$45.00, paper \$16.95.

Leo P. Ribuffo has collected six articles that he published between 1980 and the present, added a new one, and framed the whole with a thoughtful historiographical essay. The pieces range in subject matter and scope. One is a sweeping and synthetic look at American religious prejudice. Two reassess the controversies that have swirled over Henry Ford and Bruce Barton. Ribuffo places both of these emblematic twentieth-century Americans in the Progressive tradition. Of the other essays, one seeks to "normalize" American communism within the contours of American politics, another attempts to argue the limits of the New Deal by analyzing the Federal Theater Project's production of Sinclair Lewis's *It Can't Happen Here*, and two final pieces try to explain the losing presidencies of Gerald Ford and Jimmy Carter. Somewhat unexpectedly, Ford gets more sympathetic treatment than Carter.

Ribuffo writes well, and his perspective is always challenging and refreshing. Several themes tie the essays together. The first is Ribuffo's interest in religion, which he believes is a subject that American historians commonly get wrong. Ribuffo is no champion of religious belief. He agrees with other historians that religion has been at the center of much nasty prejudice between and within ethnic groups. He observes with considerable accuracy, however, that

historians, when confronted with dangerous attitudes such as nativism and anti-Semitism, often assign the people who hold those views to a lunatic fringe. "Counterprogressive" historians tended to make popular religion pathological along with everything else that deviated from cosmopolitan rational norms. In Ribuffo's view, we often explain away or ridicule what we have not yet understood. To think that "paranoia" is a useful explanation of religious fundamentalism is the intellectual's way of buying false security.

In a more general sense, Ribuffo argues that historians of all stripes, and he includes the New Left historians of his own generation, tend to "exoticize" or make eccentric what is run-of-the-mill American. It was not only economically marginal people who found Henry Ford's anti-Jewish diatribes in the *Dear-born Independent* believable. The opinions spoke to mainstream America. Bruce Barton's life of Christ was not an outrageous piece of capitalist demagoguery. Its point of view emerged from the liberal Social Gospel. American communism in the 1930s and 1940s was not a crazy form of political extremism that appealed to lonely people without firm social moorings. People joined and left the Communist Party for much the same reasons that they joined and left other political parties. Communists were no more naive about the Soviet Union in the 1930s than average Roosevelt Democrats were about Nazism.

Ribuffo is not neutral about his subjects. His efforts to reinterpret some standard villains of twentieth-century American historical writing is not done to win admiration for them. Insofar as there is heuristic intent, it is to make us more critical, much more critical, of the individuals and processes that are usually assigned the correct and progressive roles in American life: liberal secularists, for example. Ribuffo leans left without being particularly romantic about anything, not even populism. He rescues his subjects from the "enormous condescension" of historical judgments in the simplest possible way: he turns them into our neighbors. These are acute essays, and entertaining ones as well.

R. LAURENCE MOORE
Cornell University

MARTIN J. SKLAR. *The United States as a Developing Country: Studies in U.S. History in the Progressive Era and the 1920s*. New York: Cambridge University Press. 1992. Pp. xi, 238. Cloth \$49.95, paper \$14.95.

Martin J. Sklar's study consists of seven essays spanning his career, including one new piece written specifically to introduce this book. The common thread to these essays, three of which have been previously published, relates to the theme of "modernization," that is, of the United States as a developing nation. The focus is on the period from 1890 to 1920.

Sklar, who describes himself as "a true student of

the "Wisconsin school'" (p. x), argues that American life around the turn of the century underwent a transformation from an older "proprietary-competitive market" stage to an emerging "corporate administered market" stage. The reorganization of "corporate capitalism" grew from traditions of small-producer capitalism that mixed together with traditions of populism and socialism. This reorganization constituted the substance of development and "modernization." Corporate capitalism did not represent a static economic structure against which a variety of oppositional social movements then took shape. (He criticizes bottom-up social historians for operating from such an assumption.) Rather, he argues, these emerging economic institutions themselves arose out of the interplay among various social and intellectual movements. The corporate reconstruction of capitalism involved new modes of both production and consciousness.

Sklar sketches this broad interpretation in the recently written first two essays, which are then succeeded by narrower investigations chronologically following his career. A previously unpublished essay from his Master's thesis, on the ideology of dollar diplomacy diplomats, and his widely cited 1960 essay, originally published as "Woodrow Wilson and the Political Economy of Modern United States Liberalism," provide specific examples supporting his view that corporate capitalism emerged from a social movement in which intellectual and moralistic commitments fused together with a program of economic development. Although both essays represent Sklar's work as a graduate student over thirty years ago, they remain insightful studies. Many scholars will welcome their new accessibility.

Other essays fit less comfortably with Sklar's overall thesis but illuminate more about the twists and turns of his own radicalism. In an essay on the "disaccumulation of capital" in the 1920s, written in the late 1960s, Sklar analyzes the limitations of the radicalism espoused by the "young intellectuals" of the 1920s. Using a heavily deterministic Marxist framework, he calls for "mental workers" of his own day to realize their historic opportunity to join with other workers in forging a fully class-conscious proletariat and bringing revolutionary transformation to bourgeois society.

The next essay, focusing on Henry Adams and written thirteen years later, in the early 1980s, reflects Sklar's soul-searching over loss of radical direction, his alienation at feeling caught "between ages," detached from, rather than part of, history. Sklar still calls for emergence of a "revolutionary intelligence," but he also hopes to summon the personal courage to "acknowledge acquiescence" to a flawed capitalist order that should have fallen but did not (p. 208).

In writing *The Corporate Reconstruction of American Capitalism* (1987), Sklar apparently found that acquiescence. The last essay, an extended reflection on that book, emphasizes his conclusion that American devel-

opment mixed public and private, socialism and capitalism. No longer calling for revolutionary class consciousness, Sklar in the early 1990s nonetheless still invokes historical materialism and Marx, this time to help investigate what he calls "The Mix" (of socialism and capitalism) that made the United States the most stable, dynamic, and progressive of nations.

This book interprets American development in the early part of the century and offers intriguing perspectives on modernization and periodization; it also illuminates Sklar's own intellectual odyssey from outrage and despair over American life to acceptance and almost celebration.

EMILY S. ROSENBERG
Macalester College

STEPHEN MEYER. *"Stalin over Wisconsin": The Making and Unmaking of Militant Unionism, 1900-1950*. (Class and Culture.) New Brunswick, N.J.: Rutgers University Press. 1992. Pp. xi, 265. \$45.00.

After generations of investigation by scholars from several disciplines, the character and significance of the industrial union movement that emerged in the 1930s remains contested terrain. Labor economists whose world view was shaped by the New Deal era continue to believe that the Congress of Industrial Organizations (CIO) represented a sharp break with the unionism of the American Federation of Labor (AFL) and a major step toward industrial democracy. Other scholars, influenced by the New Left of the 1960s, tend to see the CIO as a bureaucratic encumbrance on the rank-and-file insurgency of the 1930s. And coming full circle to embrace the theories of the first generation of labor economists and historians, still others have argued that the CIO quickly became a reflection of the basic "job consciousness" of American workers.

In this book, Stephen Meyer makes an important and original contribution to this ongoing debate. Meyer emphatically agrees that the CIO represented a major departure from the essentially conservative and elitist traditions of the AFL. But unlike most others who have made this point, he roots his argument in a skillful dissection of the "hidden and contested terrain of the workplace" (p. 14) at the Allis-Chalmers Manufacturing Company, where the shop-floor practice of United Auto Workers (UAW) Local 248 represented "the essence of militant CIO unionism" (p. 105). Allis-Chalmers was an important manufacturer of agricultural equipment. Until the 1930s its management had successfully resisted unionization. But the Great Depression and the New Deal created a new set of conditions that made some form of unionism inevitable at the company's huge West Allis Works; and although there were many contestants for the prize, a group of left-wing unionists succeeded in organizing the plant under the banner of the UAW and the CIO.

Meyer readily concedes that most of Local 248's leaders were either members of the Communist Party or loyal "fellow travelers." But unlike many others who have studied CIO unionism, he is little interested in elaborating on this fact. Rather, he shifts the focus to what the union attempted to accomplish on the shop floor; and here his contribution is of real value. First, he demonstrates that Local 248's leadership succeeded in building a dense network of stewards and committeemen in the plant, and a strategic sense of how to use the grievance procedure, that quickly became an effective institutional structure to defend workers' rights and challenge management's prerogatives. Second, while acknowledging that the union leadership accepted the notion of a "workplace rule of law," he disputes the view that this regulatory network and its administrative processes necessarily constituted an imposition from above that ultimately undermined workers' militancy at the point of production. In contrast to New Deal technocrats, corporate officials, and even other CIO unionists, Local 248 leaders saw the grievance procedure and arbitration as a way of focusing but not containing class struggle. On a day-to-day basis, as Meyer puts it, "they bargained collectively, obtained contracts, created systems of shop floor representation, and forcefully challenged managerial authority at the workplace" (p. 221).

But precisely because they did all of this so aggressively and effectively, they became an "intolerable challenge" (p. 106) to management's authority. Meyer demonstrates that as the political climate in Wisconsin and the nation as a whole shifted to the right in 1946, a wide array of forces combined to impose a crushing defeat on the Allis-Chalmers unionists, who at the very moment that Joseph McCarthy was waging a successful campaign for the U.S. Senate were themselves in the midst of a long and bitter strike. Meyer argues that "McCarthyism" was born "in the fire of Milwaukee labor politics" (p. 178).

By focusing on the substance of labor-management conflict on the shop floor and at the bargaining table, however, he makes it clear that the major strikes of 1939, 1941, and 1946-47 were each rooted in issues of wages, grievance resolution, and—above all—union security and not, as the Local 248 leadership's opponents charged, in the oft-changing agenda of the Communist Party. This is an exceedingly important point, but unfortunately Meyer turns a strength into a weakness by so relentlessly separating the Local 248 leaders' shop-floor objectives from their support of the Communist Party's larger political agenda and then largely ignoring the latter. He thus leaves unanswered a number of important questions about how the Allis-Chalmers experience compares to other examples of Left-led unionism during the same time period. Nonetheless, Meyer has written a fascinating case study that will be of real value to students of

industrial relations, labor, the Left, and New Deal and Cold War politics.

BRUCE NELSON
Dartmouth College

MICHAEL E. PARRISH. *Anxious Decades: America in Prosperity and Depression, 1920-1941*. (Norton Twentieth Century America Series.) New York: W. W. Norton. 1992. Pp. xiv, 529. \$29.95.

Michael E. Parrish's book is the latest release in the Norton Twentieth Century America Series, edited by John Morton Blum. Intended for both lay and professional readers, it tells the dramatic story of the period between World War I and World War II in the United States. Although Parrish intends his book to be a "broad historical interpretation" (p. xiii), its strengths lie in its ambitious synthesis of recent historical literature rather than in any new understanding of the era.

Parrish is the first historian in many years to treat all aspects of the interwar years in a single volume. Along with classic accounts, he synthesizes large amounts of recent work in African-American and women's history, popular culture and media studies, labor history, and political criticism. He also draws important connections between the 1920s and 1930s, so often treated as contradictory periods, as well as between those years and the eras directly preceding and following them. Parrish contends, for example, that the shift from a producer to a consumer ethic that took place in the 1920s did not disintegrate in the 1930s, but rather was kept alive through government policy, religion, and film, and thus was ready to take full form in the postwar years. This shift, together with the increasing bureaucratization of all aspects of American life and the inevitable internationalization of foreign affairs, made the interwar years among the most important "sources of our contemporary world" (p. ix).

The book's weaknesses are often the flip-sides of its strengths. In a clear, witty, and engaging style peppered with unforgettable details, Parrish brings to life many of the main characters of the period. Warren Harding, for example, "was a swell guy . . . He had slapped many a back, played innumerable games of stud poker, and hoisted his share of cocktails" (p. 8). Andrew Mellon, however, "resembled the village undertaker" (p. 16). Yet other, less-important players—women and rural Americans in particular—appear more often as lists, trends, or mere conundrums than as multifaceted individuals. Much more frustrating, however, is Parrish's unrelenting even-handedness in his treatment of the competing historical interpretations of the era. Harding's record, he tells us, for example, was not "simply corrupt, mediocre, and reactionary" but "somewhat more mixed" (p. 13). Herbert Hoover likewise was both "'the first of the new Presidents'" and "'the last of the old'" (pp.

242–43). Franklin Roosevelt's New Deal too "contained both radical and conservative elements" (p. 271). By continually trying to find a middle ground between conservative and liberal interpretations, Parrish successfully avoids oversimplification. At the same time, however, he becomes more like a referee than a player in his own right. Ultimately, his own interpretation seems far too comfortable, familiar, and reassuring for a period of such tremendous significance. Parrish admits that "people do not always agree about the past" (p. 437). Ironically, he has written a book about which nearly everyone will agree, but almost no one will get excited.

CATHERINE McNICOL STOCK
Connecticut College

DAVID WARREN SAXE. *Social Studies in Schools: A History of the Early Years*. (SUNY Series, Theory, Research, and Practice in Social Education.) Albany: State University of New York Press. 1991. Pp. xvii, 310. Cloth \$57.50, paper \$18.95.

This book by David Warren Saxe examines the origins of social studies in the school curriculum from the 1880s through 1916. Although Saxe writes about a neglected subject, his inquiry is important because it deals with how Americans, through schooling, developed their perspectives on the historical past and the social, political, and economic present.

Saxe's historical narrative re-creates the interaction between the broad contextual forces and major personalities that led to organized efforts to shape curriculum and instruction first in history and then in social studies. The currents of Social Darwinism; social efficiency, pragmatism, and Progressivism provided the contextual milieu in which major players such as Arthur Dunn, Edmund James, Henry Johnson, Clarence Kingsley, David Snedden, James Harvey Robinson, and others impressed their educational agendas on the emerging social studies. The curricular designs shaped by this interaction were devised by the organized efforts of committees such as the Madison Conference associated with the Committee of Ten, the Committee of Seven, and the Committee on Social Studies of the Commission on the Reorganization of Secondary Education. By analyzing the interaction between context and actors, Saxe first describes history's entry and then dominance in the secondary school curriculum from 1880 through 1910. To win a place in the curriculum, history had to displace the hitherto dominant classics. History's place in the secondary curriculum was protected by the report of the Committee of Ten. During the Progressive Era, history's dominance was challenged by the newer social sciences such as sociology, economics, and anthropology, known first as social education, and then retitled social studies. Saxe thoroughly examines the role of the leading social efficiency educators, David Snedden and Clarence

Kingsley, whose social studies alternative challenged traditional history. This challenge was embodied in the Committee on Social Studies' report in 1916.

Saxe's analysis of the struggle for time and place in the curriculum offers illuminating insights into current social studies controversies. Historians are still uneasy with the loose definitions and eclectic scope of social studies education; social studies educators still feel constrained by history's chronological structures. Many of the issues that surfaced during the period from the 1880s to 1916 remain with us.

Saxe's carefully researched, well-organized, and clearly written narrative should be read by those concerned with preparing history and social studies teachers. It clearly explains how the social studies curriculum reached its present development. For historians, it recounts history's entry into the curriculum and its redesign to fit the contours of changing educational situations shaped by the impact of Herbert Spencer's Social Darwinism, David Snedden's social efficiency, John Dewey's Experimentalism, and Progressivism. For social studies educators, it illustrates how the same forces merged with such newer social sciences as sociology, anthropology, and economics to create a new curricular field. What appears is a sense of two parallel but not integrated parts of the same curriculum—one history and the other social studies. Although the social studies insurgents emerged victorious in 1916, the battle would be refought many times.

The book, a needed contribution to an important but neglected area, is enhanced by copious and highly useable citations and an extensive bibliography that leads the scholar to the literature in the field. It also has valuable appendixes that include *The Teaching of Community Civics* (1915) and *The Social Studies in Secondary Education* (1916), part of the work of the Commission on the Reorganization of Secondary Education. The book also has an accurate index.

The book is the first of what Saxe promises will be a trilogy on the history of social studies. If the future volumes meet the quality of the first, they are eagerly anticipated.

GERALD L. GUTKIN
Loyola University of Chicago

RICHARD M. FREELAND. *Academia's Golden Age: Universities in Massachusetts, 1945–1970*. New York: Oxford University Press. 1992. Pp. xii, 532. \$49.95.

Richard M. Freeland's book links institutional saga to organizational behavior, local history, and public policy by means of a fascinating study of universities in Massachusetts during the quarter-century following World War II. His focus is a significant case by which to examine the "multiversity" model Clark Kerr described thirty years ago in *The Uses of the University* because higher education in Massachusetts was both influential and atypical. Unlike California, for exam-

ple, Massachusetts's postwar economy was not booming, nor did its state government have a tradition of strong funding for its public university. Although Massachusetts had long been known for liberal arts colleges, one of the state's major contributions to American higher education was that the Boston area was host to a rich concentration of independent universities: Harvard, MIT, Tufts, Boston College, Boston University, Northeastern, and the newly founded Brandeis.

The essential dynamic was that after 1945 presidents and boards at each of these universities were driven by pursuit of prestige in competition for resources and students. By 1950 the competitive arena was expanded to the rural west as the University of Massachusetts gained belated political support. The paradox of prosperity was that as each institution sought its niche, all converged on a similar organizational pattern: the sprawling structure of the decentralized comprehensive research university. A price to be paid for this success story was that some presidents ventured on a fool's errand to make the modern university simultaneously "elite" and "all things to all people." As Freeland quotes from a memoir about one institution's plans in 1951, "There was no objection to any professor or department being first rate, and there was no encouragement either. The administration was benevolently neutral. It was a mediocre place, but it was like a fruitcake, and it was studded with wonderful bits and pieces" (p. 247).

Freeland's study goes beyond campus "house histories" by connecting his institutional profiles to national trends, and he is careful to return to his original questions about organizational behavior and university mission. His historical research elaborates on themes advanced by sociologists Christopher Jencks and David Riesman in their classic *The Academic Revolution* (1968) and emphasizes gains in faculty power within universities. But even Freeland tempers his interpretation by noting that much of the alleged faculty strength wilted when research grants and academic job markets subsided after 1970. His epilogue leaves the sobering impression that universities in Massachusetts spent the golden age preoccupied with a base metal: the brass ring of external funding, whether from federal grants, foundations, or donors. Freeland amends the simplistic explanation that outside pressures dictated the campus agenda; rather, quests for new money often were opportunistic strategies to subsidize priorities already favored within the institution. One disheartening corollary is that many universities in Massachusetts relied on selective undergraduate admissions to enhance general institutional prestige and to increase tuition revenues less as a commitment to improving undergraduate curriculum and more as a ploy for gaining resources for graduate programs. The decades following World War II were marked by unexpected consequences and overextension during which the campus presidency ascended and then

experienced a loss of coherence and control. Despite unprecedented prosperity, the tales of campus campaigns seem grim, suggesting that higher education from 1945 to 1970 lacked the excitement that characterized heroic university-building from 1870 to 1910.

JOHN R. THELIN
Indiana University,
Bloomington

JAMES L. BAUGHMAN. *The Republic of Mass Culture: Journalism, Filmmaking, and Broadcasting in America since 1941*. (The American Moment.) Baltimore: Johns Hopkins University Press. 1992. Pp. xvii, 257. Cloth \$38.95, paper \$11.95.

James L. Baughman's study of U.S. mass culture in the last half-century successfully integrates media content, commerce, technology, and external influences and brings the reader to the banal state of mass media by 1990. The book addresses radio, television, newspapers, periodicals, and films and the powerful but shifting interrelationships among these areas. The book's great strength is how Baughman traces the interconnected web of the established media and the emergent medium of television.

The main outlines of modern print journalism such as the eight-column page were established in the nineteenth century and continued in the twentieth century. The power of newspapers waned by the 1940s, however, as radio and, most importantly, television began to shape U.S. media. In the competitive flurry of the 1950s, newspapers revived past practices used by periodicals to secure reader loyalty such as offering service and advocacy features for readers. At that time, joint operating agreements where newspaper competitors joined their "printing and business operations while maintaining separate news staffs" (p. 123) were another device borrowed from other industries. Even so, evening dailies declined and then disappeared with the increasing popularity of car radios that served the captive audience of the automobile commuter.

As markets changed and readership declined, newspapers innovated in offering zoned editions of daily papers in order to reach specialized suburban markets. Baughman says that newspaper interest in national and international coverage waned, but they remained the chief source for news programming by television networks.

The general interest magazine also lost ground and only the highly specialized succeeded in the new markets. The dependence of the print media on television was evident by the 1960s when the most popular magazine in the United States was *TV Guide*. In the competitive periodical market, more than circulation was necessary; in the new era, advertisers demanded evidence that a magazine was read.

Baughman shows how television's strength trans-

formed information distribution not only through the television networks but also by its impact on radio and print journalism. Television news coverage was no more than a headline service that provided brief packaged segments. News magazines such as *Time* succeeded in the late 1960s because their style and content were acceptable to a television audience readership accustomed to brief news reports.

Public affairs programming in all media was high in cost, its returns low, and its ratings poor. For example, the pioneering television documentary disappeared by the mid-1960s. Arts programming, too, on Sunday afternoon collapsed and was replaced by the networks with sports programming.

Integrating the many components of mass media into a general overview of U.S. culture, Baughman says that by the late 1980s, the widely touted "Information Age" was an illusion because extensive superficiality prevailed in print and broadcast media. For example, investigative reporting in the post-Watergate era cheapened into a search for scandal. Quality issues too often were neglected by the barrage of bad programming. Upper-middle-class audiences touted a slick soap opera from the BBC, *Upstairs, Downstairs*, that was entertaining in the context of usual network fare.

Baughman provides a useful annotated bibliography organized by chapter and thus mollifies the reader who searches in vain for footnotes or endnotes. His book is an important contribution to the history of the media industries.

SALME H. STEINBERG
Northeastern Illinois University

WILLIAM B. HIXSON, JR. *Search for the American Right Wing: An Analysis of the Social Science Record, 1955-1987*. Princeton: Princeton University Press. 1992. Pp. xxvii, 357. \$49.50.

William B. Hixson, Jr.'s book is not one for the faint of heart. It is a dense *Baedeker's* to aid explorers of the four main manifestations of the American right wing after 1945: McCarthyism; the "radical right" of 1958-64 exemplified by the John Birch Society and the presidential candidacy of Barry Goldwater; the George Wallace phenomenon from 1964 to the early 1970s; and the rise of the "new right" that culminated in the election of Ronald Reagan and in the cultural politics that stressed issues of gender, sexuality, and family.

The book primarily offers an atlas, assembled via careful, detailed analysis and comparison of the various theories elaborated by historians, political scientists, sociologists, and psychologists, to explain these developments. Hixson terms his contribution a "creative synthesis" made necessary by the fact that so many studies of this period refer to this literature in ill-informed, second-hand ways. Additionally, the au-

thor provides a useful mapping of his own of the more recent of these phenomena.

The section on the bases of Joe McCarthy's support focuses on theories of "status politics" and McCarthyism's "populist" aspects first collected in Daniel Bell's *The New American Right* (1955) and on the critical responses, notably Michael Rogin's, to these "pluralist" interpretations. In turn critiquing Rogin, Hixson suggests the applicability of Robert Wiebe's "localist" interpretation to this phenomenon.

The author next surveys accounts of the "extremist" right of the late 1950s and early 1960s and assayings of the sources of Goldwater's support. Several commentators on McCarthyism—Bell, Richard Hofstadter, Seymour M. Lipset, and David Riesman—scrutinized this next phase. Hixson shrewdly describes how some adjusted their earlier approaches, primarily by downplaying "status politics" and, in Hofstadter's case, seizing on the centrality of "fundamentalism" to the shaping of the "radical right" and emphasizing the "cultural" bases of the right-wing orientation.

The third portion of the book appraises efforts of social scientists to locate the differing sources, North and South, of George Wallace's constituency. Rogin and Lipset, along with Walter Dean Burnham, emerge as the key commentators on this phase.

Hixson's treatment of the "new right" of the 1970s and after reviews a profusion of studies on the rise of new right-wing organizations led by Paul Weyrich, Howard Phillips, and others; the activation of the religious right under Jerry Falwell and company; the efforts of "pro-family" coalitions against the Equal Rights Amendment and other perceived threats; and the capture of the Republican Party by these forces.

Finally, Hixson searches for an overarching synthesis. He criticizes the literature for a piecemeal quality, for the way scholars have talked past each other, pursued disparate research agendas, and thus obstructed grand interpretation. He singles out Rogin's and Lipset's work as having a breadth that might lead to a broad explanation of rightist activity in general, but he painstakingly locates shortcomings in both. He turns last to "modernization" theory as adumbrated by Richard D. Brown, with a coda devoted to Richard Jensen's suggestion of a "postmodern" sequel to modernization, to provide a degree of scholarly closure.

The book is a comprehensive and valuable guide to the literature. Omissions are few and tend to be among works that emphasize pre-1945 right-wing manifestations. Hixson's analysis is exhaustive and essential to students of the post-1945 right wing.

RICHARD M. FRIED
University of Illinois,
Chicago

ALEXANDER CHARNS. *Cloak and Gavel: FBI Wiretaps, Bugs, Informers, and the Supreme Court*. Urbana: University of Illinois Press. 1992. Pp. xviii, 206. \$24.95.

There is both more and less to Alexander Charns's brief but provocative monograph than meets the eye, more in the sense that his tenacious, patient, lawyerly pursuit of the Freedom of Information Act (FOIA) represents a triumph in litigation, discovery, and historical sleuthing. The chapter endnotes alone are worth the price of the book. There is less, however, in the sense that revelations of the J. Edgar Hoover-led Federal Bureau of Investigation's half-century of indiscretions, improprieties, and collusion with the executive branch may strike some readers as numbingly familiar, disturbingly commonplace, and even tantalizingly circumstantial. The fact that the prose alternates between a precise, well-crafted, brief-like tone and an unfortunate similarity to melodramatic potboilers promising to reveal all creates an uneven quality that detracts from the book's substantive warnings about the fragility of civil rights and liberties. At his best, Charns's lean prose gracefully elucidates some complex legal issues; on other occasions, sarcasm and docudrama are unwelcome distractions.

For readers who may be unfamiliar with FBI Director Hoover's fondness for "black-bag" jobs, "EL-SUR" (electronic surveillance), "June Mail" (fruits of illegal activities), and confidential "do not file" files, this book should rip away the last remaining vestiges of naiveté and idealism surrounding the putative separation of powers that Americans presume exists between the executive and judicial branches of their government. Other readers more familiar with abuses of the Fourth Amendment such as warrantless eavesdropping and illegal wiretapping, as well as demonstrated collusion between the FBI and congressional investigative committees, may conclude that this compendium of tantalizing and occasionally circumstantial evidence, some of it based on third-party conversations, undocumentable telephone logs, and the FBI's own notoriously inaccurate files, promises more than it delivers.

As in all historical works, there are winners and losers, onlookers and straw figures, and the names are frequently interchangeable depending on the historical moment and issue. The "good guys" include Supreme Court Chief Justice Earl Warren, associate justices William O. Douglas (most of the time) and William Brennan, Jr., attorneys general Nicholas Katzenbach and Ramsey Clark, and any other public servant who refused to exchange in the coin of the realm—influence peddling and trading on favors—and instead insisted on maintaining the rule of law. Although Jimmy Hoffa and Robert Kennedy make cameo appearances, and Lyndon Johnson and Richard Nixon play supporting roles, true venality is limited to Hoover and the director's closest associates, and most especially, to Associate Justice Abe Fortas, the quintessential Washington insider.

The man whom Hoover once called a "sniveling liberal" emerges through the pages of this book as the FBI's and White House's mole extraordinaire, an unprincipled, unethical, and shallow operative who

betrayed his principles, informed on colleagues, and lied to his country. Charns's attacks are so unsparing that readers may wonder about the absence of balance, nuance, and complexity. Whether or not the author has uncovered what one pre-publication reviewer, Geraldo Rivera (an interesting choice), terms "a scandal of historic proportions," this book will keep FOIA administrators occupied for years to come. This is a work whose subject demands attention, dispassionate scrutiny, and vigilance.

EUGENE M. TOBIN
Hamilton College

RICHARD H. KING. *Civil Rights and the Idea of Freedom*. New York: Oxford University Press. 1992. Pp. xii, 269. \$35.00.

This book achieves much while promising more. Attempting to provide "a political-theoretical analysis of [the civil rights movement's] rhetoric and thinking" (p. 10), Richard H. King offers some thoughtful observations on the ideas of selected black activists of the 1950s and 1960s. He acknowledges the continuities between "the Southern civil rights movement" and the "black pride and black consciousness movements which appeared in the late 1960s and early 1970s" (p. 5), but his understanding and insights are more evident regarding the former. Although King occasionally uses oral history sources and includes a brief discussion of the distinctive experiences of black women activists, his study focuses mainly on the writings and public statements of articulate male political leaders and ideologues.

Richard King devotes two chapters to a sympathetic assessment of the ideas of Martin Luther King, Jr., who "was able to capture the attention, even devotion, of black Southerners and then move them to a new place psychologically and politically because what he said and the way he said it resonated so powerfully with their own experience and aspirations" (p. 88). The author argues that his namesake reflected an African-American political culture which affirmed notions of freedom that differed from traditional liberal conceptions of civil rights. Seeking more than freedom from racial discrimination or other arbitrary restrictions—the liberal definition of civil rights—black leaders have historically struggled to liberate themselves from the values, attitudes, and social relationships associated with racial oppression and dependency. African-American conceptions of freedom, the author suggests, extend beyond Isaiah Berlin's concept of negative freedom or natural liberty and are similar to Hannah Arendt's notion of freedom as political action.

Although this book provides a perceptive assessment of a major theme of African-American history, the book's discussion of King and the early leaders of the Student Nonviolent Coordinating Committee is more convincing than its unsympathetic handling of

the rise of black power militancy. King's treatment of African-American political thought of the late 1960s consists mainly of a critique of Franz Fanon's theory of violence as a form of political action and psychological therapy. In contrast to his extended treatment of Fanon, Richard King has little to say about the ideas of Malcolm X, Amiri Baraka, Stokely Carmichael, and others who greatly influenced black political thought during the late 1960s. His hostility toward the writings of Fanon and the violence-tinged rhetoric of a few black firebrands prevents him from recognizing the extent to which the black power slogan symbolized a return to traditional African-American thought.

King concludes with the observation that "we have yet to find an appropriate vocabulary to describe the civil rights movement" (p. 201), but he, like many other scholars, fails to recognize the implications of his own insights about the centrality of freedom as a concept in modern black freedom struggles. Scholars have rarely broadened their studies of modern African-American political thought to encompass the broad range of black freedom struggles of the 1950s and 1960s. Therefore, like King, they have given insufficient attention to the constant interaction between northern and southern black militancy or to the creative tension that has always existed between racial separatism and civil rights advocacy as themes of African-American politics. This study is an important critique of conventional liberalism informed by a body of modern African-American political thought that has yet to be comprehended in a single scholarly work.

CLAYBORNE CARSON
Stanford University

NICOLAUS MILLS. *Like a Holy Crusade: Mississippi 1964—The Turning of the Civil Rights Movement in America*. Chicago: Ivan R. Dee. 1992. Pp. 222. \$22.50.

Nicolaus Mills explains in his introduction why he has added to our crowded shelf yet another book about the summer of 1964 in Mississippi. From the gloomy perspective of the 1990s, with American race relations in turmoil, he promises to reexamine the events of 1964 to explain the crucial "turning" of the civil rights movement from triumph in 1964–65 toward racial separatism, violence, and eroded national consensus.

Mills interviewed more than forty participants in "Freedom Summer," but otherwise he relies on the same body of memoirs, oral histories, Student Non-violent Coordinating Committee (SNCC) and Martin Luther King papers, and FBI and Lyndon B. Johnson Library files that informed the archivally based scholarship of the 1980s—Clayborne Carson's *In Struggle* (1981), David J. Garrow's *Bearing the Cross* (1984), Doug McAdam's *Freedom Summer* (1988), and Seth Cagin and Philip Dray's *We Are Not Afraid* (1988).

Mills acknowledges the generosity of Carson, McAdam, and Garrow for sharing their evidence. He believes that Freedom Summer carries lessons about turning points that a present-minded revisionism may illuminate. The introduction concludes with a preview of his revisionist thesis: "the real tragedy of the Mississippi Summer Project is not that it failed but that so many of its participants gave up on it before its triumphs became clear" (p. 26).

The narrative conventionally emphasizes the oppression of white violence in Mississippi; the decision by grass-roots activists to recruit and train northern college volunteers; the murders of James Chaney, Andrew Goodman, and Michael Schwerner; and the climactic fight over seating the Mississippi Freedom Democratic Party (MFDP) delegation at the Democratic national convention in Atlantic City. Mills tells the story well, concentrating on the internal debate over civil rights goals and strategy and generally avoiding distractions (including interracial sex).

The clarity of Mills's argument becomes confused, however, in his crucial chapter on the MFDP challenge in Atlantic City. At first Mills seems to agree with Bayard Rustin, Roy Wilkins, Joseph Rauh, and Allard Lowenstein that the compromise offered by President Johnson's forces would maintain national party unity against Barry Goldwater and break the segregationist hold on the party in Mississippi (which it did). The liberal defenders of the compromise argued that the MFDP was so isolated and traumatized by racial terror in Mississippi that it could not comprehend the nature of the American political system and was unable to recognize its own victory. As Lowenstein observed, SNCC was polarizing toward a revolutionary reductionism that alienated its allies: "You're either a true believer or a sellout" (p. 160). Mills then appears to shift, however, to agree with Bob Moses, Fannie Lou Hamer, James Forman, Cleveland Sellers, and Julian Bond that the Atlantic City compromise proved the irredeemable corruption of the nation's political system. The author seems to agree with Moses that the Democratic convention "fatally undermined the arguments that had been advanced within the Summer Project for alliances with white liberals" (p. 191), and made it impossible to refute Stokely Carmichael's charge that "This proves that the liberal Democrats are just as racist as Goldwater" (p. 163).

Mills argues both ways. He attacks as racial scapegoating the "current orthodoxy" of "once-liberal historians" who blame the SNCC militants of 1964 for leading the movement down the road to black nationalism, revolutionary posturing, and racial polarization. Yet he deplores the degeneration of the integrationist, democratic SNCC into a cult of personality under Carmichael and H. Rap Brown under which "racial hatred became fashionable." In his epilogue, Mills leaps from 1964 to the 1990s and affirms the social democratic critique of William Julius Wilson that racial preference is the enemy of class-based

coalitions. But he fails to pursue systematically the logic of his revisionist vision from the perspective of the Democratic Left. Why did liberals so easily abandon their principle of a color-blind Constitution and condone racial separatism and even violent protest? What deterministic assumptions about civil rights politics and policy during the 1960s need reexamining in light of our subsequent experience? What alternative choices were possible, and what lessons can we learn to guide our public life? Mills gives us a brief, suggestive epilogue, but he has earned, and readers deserve, the harvest of a thoughtful concluding chapter that his introduction led us to expect.

HUGH DAVIS GRAHAM
Vanderbilt University

JOHN MORTON BLUM. *Years of Discord: American Politics and Society, 1961–1974*. (Norton Twentieth Century America Series.) New York: W. W. Norton. 1991. Pp. x, 530. \$25.00.

This is, in many ways, a wise and melancholy book, one that should be known to every student of what increasingly looks like the most pivotal era of the recent American past. John Morton Blum's view of the Kennedy-Johnson-Nixon era, that interregnum between the last gasp of New Deal liberalism and New Right triumph, reflects more despair than hope. When it was all done, when all the opportunities were kicked away, whatever gains came out of the "years of discord" could be sustained only by, as Blum sees it, more pristine liberal aspirations. Presumably, if one reads him correctly, his greatest lamentation is over opportunities lost, and they were forfeited for precisely the lack of ideological purity that later enabled Ronald Reagan to drive home the conservative message. Even Blum's view of the period, however, acknowledges that the three administrations central to his account "successfully employed the large powers of the presidency in ways that made the state a threat to Americans" (p. 477).

He does make clear that the problem was less in the deployment of that power, less in the reality of an "imperial presidency," than in how they used that power. Blum's implicit difference with such critics as Charles Murray and fellow neoconservatives holds that Kennedy and Johnson, far from pursuing impossible and even counterproductive goals, had little appetite for using that power for the illusive ends of liberal redistribution of money and resources. The Kennedy campaign of 1960 "provided a rough political test of strength between the complacent and the concerned." But Kennedy's failure of resolve left him between the mainstreamers and the reformers. Safety, for JFK, was a course that kept him closer to the Eisenhower vision than to New Left idealism. Radicals of the 1960s "were evangelical, and he was the rector of the established church. They were passionate; he was the embodiment of the cool" (p.

101). Johnson, child of the New Deal, was nevertheless disinterested in relieving the burdens of the working poor and, despite his Rooseveltian visions, happily supped with the money-changers in their temple. Nevertheless, Blum emphasizes, despite that pro-business weakness—maybe because of it—Johnson's success was sufficient to draw the United States, for a time at least, closer "toward the company of other Western nations in the range and energy of its treatment of the poor" (p. 177). The Great Society, underfunded and further decimated by Vietnam, still did succeed in creating programs that helped the poor.

The vulnerabilities of Kennedy and Johnson in their pursuit of wars against poverty, communists everywhere, and segregation at home, were matched by the flaws of the Warren Court, which needlessly, and carelessly, "played into the hands of conservatives determined to see the decisions overturned." Blum, in an admission rare for a liberal historian, concedes that "Qualities of haste in drafting legislation and of sloppiness in administration damaged the functioning of the Great Society . . . Their opinions lacked the judicial craftsmanship to protect them from future modification . . . That vulnerability played into the hands of conservatives determined to see the decisions overturned. And that political threat endangered the democratic results of the Warren Court's rulings" (p. 216). As a result, Nixon "did not have to invent his 'silent majority,' his Middle Americans. They were waiting for him" (p. 479). Blum's Nixon was no herald of Reagan but one who stood between the social activism of the Democratic era and the advent of the Republican right. Nixon's New Federalism attempted to package New Frontier-Great Society activism for conservative tastes. In the end, "Neoconservatism in foreign policy and on social and economic issues pushed both parties toward the right—the Democrats away from liberalism; the Republicans away from Nixon" (p. 460).

Several disconcerting errors unfortunately mar the work. James Tobin, not Walter Heller, was the Kennedy economist who later won a Noble Prize. Kennedy's housing desegregation order was muffled by being slipped in before the Thanksgiving holiday in 1962 and not on the last day of the month. Nevertheless, this is a wise book by a distinguished historian, one sufficiently suggestive to nurture several seminars.

HERBERT S. PARMET
City University of New York

JOHN ROBERT GREENE. *The Limits of Power: The Nixon and Ford Administrations*. (America since World War II.) Bloomington: Indiana University Press. 1992. Pp. xvi, 296. \$29.95.

John Robert Greene's skillful survey makes basic points in standard ways. He highlights "detente and

devolution" diplomacy, federal-state relations, and executive branch reorganization; treats energy, stagflation, and arms limitation matters more than most texts designed for college students do; and omits subjects such as space, environment, international trade, and new social issues and social regulations. Greene's main point is that myths of American omnipotence ended with Vietnam, and that presidents afterward became "constricted," not "imperial." Nixon failed to recognize new limits on his power, alienated and angered Congress, and thus destroyed himself and further weakened the presidency.

Greene necessarily enters more contested terrain regarding Watergate, which he theatrically terms "clearly the single most important event in the nation's postwar politics" (p. 128). His version prominently features multimillionaire recluse Howard Hughes and Attorney General John Mitchell. He claims that Hughes paid off so many noted politicians that payola produced the Watergate break-in. Nixon's men burgled "to document the relationship between [Democratic National Committee chairman Lawrence] O'Brien and Hughes and to see to it that Nixon's relationship with Hughes never saw the light of day" (p. 148). Mitchell, meanwhile, was the main political paymaster for those bribing Spiro T. Angew (pp. 166–67). Apart from these assertions, Greene interweaves standard accounts.

Social issues pose problems for Greene. Presidents Nixon and Ford are portrayed as men of the "middle": those vaguely identified middle Americans whose conservative social, economic, and political values Dwight David Eisenhower best exemplified from 1945 until his death in 1969. This "middle," a "massive segment of America," restored Nixon to power to "overthrow Johnson's Great Society," wanted to roll back the "social revolution of the Sixties," and also to restore traditional values (p. 20). Given all this, which social issues were crucial? Greene avoids clarifying. He lists films, novels, plays, murders, and protests he dislikes, then concludes that a sex and violence oriented "upheaval of the 1960's and early 1970's had a striking permanence that eclipsed any previous social protest in United States history" (p. 181). Lumping together Charles Manson, *The Godfather*, the Chicago Seven, and Andy Warhol like this falsely unifies diverse social trends. It is equivalent to molding Operation Rescue, *Ann of Green Gables*, Jim Bakker and Tammy Faye Bakker, and the latest Rambo movie into one counterrevolutionary monolith. When Greene does not specify how busing, affirmative action, abortion, birth control, and other changes alienated the "middle" and elected Nixon and other conservative presidents after him, he omits a key component of the story he wants to tell.

Greene is far better at Asian and Middle Eastern (though not Latin American or European) diplomacy. He concludes Nixon's inability to transcend bipolar cold warrior assumptions in the Middle East "was the single largest policy failure" (p. 232) of his

administration. Gerald Ford is treated sympathetically, as an honorable conservative whose foreign and domestic efforts were shredded by "feuding staff, a recalcitrant Congress, and the Republican Right" (p. 215). Overall, Greene digests many participants' memoirs and academic historical accounts. His book is a coherent and competent source that students in survey courses can use as a springboard for further study of the Nixon and Ford administrations.

KIM MCQUAID
Lake Erie College

GODFREY HODGSON. *The Colonel: The Life and Wars of Henry Stimson, 1867–1950*. Paperback edition. Boston: Northeastern University Press. 1990. Pp. 402. \$16.95.

Godfrey Hodgson's biography of Henry L. Stimson argues that Stimson's life is best understood as a progression from a youthful devotee of nationalistic imperialism, modeled after his idol Theodore Roosevelt, to the more mature statesman who strenuously insisted that the United States must become the moral leader of the world. In that process, Hodgson argues, Stimson became "the founding father and patron saint" of the American "foreign-policy establishment," which arose after World War II (p. 5).

Stimson's long career as William Howard Taft's secretary of war, Herbert Hoover's secretary of state, and, again, as Franklin Roosevelt's secretary of war during World War II linked the "foreign policy of the late nineteenth-century American imperialism to the foreign policy of the Truman, Eisenhower, and Kennedy eras; the link between Theodore Roosevelt and Dean Acheson" (p. 388). Because of his conviction that the United States must become the moral leader of the world, and because he sought out and nurtured bright young men who later became important policy makers during the Cold War, Stimson exerted a profound influence over U.S. foreign policy during the "American century." Nonetheless, while this transitional, contradictory, "Janus-faced" leader was one of the crucial designers of the post-World War II world, Hodgson believes that "every stage of his public career was characterized . . . by ambiguity" (p. 21).

Hodgson is best at explaining Stimson's character, personal beliefs, and aristocratic life style. He admires Stimson's honesty, courage, determination, and willingness to serve his country. He is fascinated by Stimson's love of the military life and the conflicts generated by his equally strong religious convictions. While generally sympathetic and admiring, the author is critical of Stimson's racism, ethnic prejudices, imperialistic impulses, support for Anastasio Somoza in Nicaragua, his indifference to social injustice and human suffering, and his superficial understanding of European affairs. The reader gains valuable in-

sights into Stimson, the self-styled aristocrat, in this well-organized and well-written book.

Although often providing extensive context for the general reader, Hodgson's study contains serious deficiencies. The most significant is his almost total reliance on Stimson's diary, papers, and published works as his primary sources. In a book with only occasional informal footnotes and no bibliography, it is evident that he has not researched the relevant primary sources on Stimson's public career. He did not, for example, consult the extensive materials at the National Archives or the collections at the Herbert Hoover Presidential Library. Instead, he relies primarily on Stimson's diary and secondary works. As a result, he is often too generous in crediting Stimson for foreign policy initiatives and too often both wrong about and unfair to Hoover. Seemingly better are his discussions on the Philippines, World War II, the decision to drop the atomic bomb, and relations with the Soviet Union, but one suspects that further research would have clarified some of the ambiguity that Hodgson believes characterized Stimson's public life.

To argue that Stimson could have been the "arbiter" of Europe in 1931–32, that he was the "Organizer of Victory" prior to U.S. entry into World War II, and that he was the "founding father and patron saint of the American foreign policy establishment" exaggerates his influence. Indeed, a great deal more research, evidence, and critical analysis on such an "establishment" is necessary before anyone is credited with its creation.

Writing a biography of Stimson is a difficult task, and given his limitations, Hodgson has written a valuable analysis of Stimson's private life and personal beliefs. Perhaps it is too much to ask that any biographer be required to research each phase of his extensive public career, but precisely because his historical importance is directly tied to his public policies and actions, one must conclude that a great deal of work still remains before Stimson's influence is fully assessed.

DONALD J. LISIO
Coe College

H. W. BRANDS. *Bound to Empire: The United States and the Philippines*. New York: Oxford University Press. 1992. Pp. xii, 384. \$28.00.

It has been over forty years since Grael A. Grunder and William E. Livezey's *The Philippines and the United States* (1951) was published. Until recently there has been only one other attempt to examine Philippine-American relations from 1898 to the present. That was *In Our Image* (1989) written by journalist turned historian, Stanley Karnow, for the mass market. It never caught on. Most Americans have no interest in Philippine affairs even when American strategic interests are involved. American consciousness about

Philippine matters can be likened to driving over hills that represent times when Americans were either fighting Filipinos in 1899–1902 or with Filipinos against Japan or the communists in the 1950s, or else bringing down President Ferdinand E. Marcos, a client who had become an embarrassment to Washington.

In their book, Grunder and Livezey attempted no analysis. They reported Philippine-American relations in over 300 pages, buttressing their presentation on published documents and studies written by former American colonial officials in the Philippines. They made no use of manuscript sources because none were available to them in 1951. Karnow used secondary works and relied on his experiences as a reporter in Southeast Asia.

H. W. Brands's book will make interesting reading for serious-minded Americans who may want to know about the country from which the fastest-growing number of immigrants from Asia are coming. He spans the entire period of American contact with the Philippines: from 1898 to the Philippine Senate's rejection in September 1991 of Washington's bid to extend the Subic Bay lease. The years before 1946 receive slightly more attention than those since independence. His book is based on manuscript sources here and abroad as well as monographic studies.

Brands's theses are measured and worked through carefully. He wants his reader to see how American and Filipino leaders, before and since independence in 1946, have worked with each other and to wonder with him for whose good have Americans operated in the Philippines. He also believes that American interaction with the Philippines can become a case study of how Washington interacts in Third World situations. Here is perhaps his strongest suit, although he does well in showing the cultural forces that are interwoven in Filipino and American relations.

Finally, Brands's study was not written for scholars who work regularly in Philippine-American affairs. If it were, we could have some discussion over nuances or about omissions. His intended audience is more general: Americans and others who might want to know how the United States came to be in the Philippines and stayed there despite Filipino objections. One hopes that this study will be required reading for collegiate courses about American power overseas since 1898.

MICHAEL PAUL ONORATO
*California State University,
Fullerton*

RODNEY J. SULLIVAN. *Exemplar of Americanism: The Philippine Career of Dean C. Worcester*. (Michigan Papers on South and Southeast Asia, number 36.) Ann Arbor, Mich.: The Center for South and Southeast Asian Studies. 1991. Pp. ix, 395. Cloth \$29.95, paper \$14.95.

Among those whom President William McKinley turned to in 1898 for expert opinion on the Philippine Islands was Dean C. Worcester, a University of Michigan professor. Worcester's academic work together with several public appearances and articles in popular journals had established him as a leading authority on the archipelago. McKinley appointed Worcester as one of the Philippine commissioners, a position he held through the administration of William Howard Taft. Worcester spent the years 1913 to 1915 in the United States campaigning for the continuance of Republican policies toward the Philippines. He then returned to the islands to resume his "career," this time as a businessman. He died there in 1924.

Rodney J. Sullivan's study is an important contribution to understanding the depth of the forces leading to American expansionism at the turn of the twentieth century, specifically regarding the Philippines. This is a rich resource because Worcester's experiences involved a variety of roles: academic researcher, policy advocate, colonial administrator, and entrepreneur. Sullivan discusses how his subject's Americanism developed from a New England upbringing and academic work associated with a mid-western institution of higher education. He then analyzes Worcester's activities from 1901 to 1913 as bearer of the white man's burden in the Philippines and emphasizes Worcester's opposition to possible changes in America's Philippine policy, especially the granting of independence. Sullivan also details Worcester's entrepreneurial activities in the Philippines. Source material for this biography includes the valuable family records preserved at the Thetford (Vermont) Historical Society, where Worcester grew up.

Sullivan concludes that in his "Philippine career" Worcester was an "exemplar of Americanism." The Americanism he exemplified was ethnocentric. Specifically, he believed that Filipinos were inferior, and that their civilization had to be brought up to American standards. In his view, when they meet, the superior civilization should not be lowered to the level of the inferior civilization. Armed with such beliefs, Worcester's activities as colonial administrator and later as entrepreneur had an important impact on Filipinos. He Americanized them, although not without having to deal with native resistance. Sullivan demonstrates that Worcester did this both in his role as colonial administrator and as entrepreneur. Sullivan is less successful in showing how Worcester influenced American strategic planning for the Philippines. Readers looking for signs of Worcester's effect on McKinley's decisions will find nothing new in this study. The meeting in December 1898 that led to his appointment as the Philippine commissioner was apparently the only face-to-face meeting between the men.

Sullivan attributes the labeling of Worcester's views and actions as "Americanism" to "countrymen—espe-

cially if they were Republicans and Manila Americans" (p. 229). To use the label Americanism to denote expansionist beliefs to the exclusion of the thinking of anti-imperialists is arguable. Sullivan locates the basis of Worcester's Americanism in values held by his New England forbears, but from these same people came prominent anti-imperialists such as Massachusetts Senator George F. Hoar. Worcester's ideology might better be called expansionism or, the pejorative used by contemporary anti-expansionists, imperialism. This issue notwithstanding, Sullivan has written a lucid analysis of the impact of an American colonial administrator, academician, and businessman on the Philippine civilization early in the twentieth century.

JAMES A. ZIMMERMAN
Tri-State University

STEPHEN J. RANDALL. *Colombia and the United States: Hegemony and Interdependence*. (The United States and the Americas.) Athens: University of Georgia Press. 1992. Pp. xii, 325. Cloth \$40.00, paper \$17.50.

Stephen J. Randall examines the diplomatic relations between Colombia and the United States from the independence of New Granada through the 1980s. The work rests on extensive material from a wide range of archives including the Ministerio de Relaciones Exteriores in Bogota. Randall attempts to write a balanced account of the relationship despite the fact (which Randall makes quite clear) that Colombia, as the weaker partner, was rarely in a position to initiate discussions and rather had to respond to them. The author attempts to give the North American reader an understanding of the aims (and internal differences) of the Colombian elites who dealt with the United States. In other ways as well, Randall attempts to write more than a standard diplomatic history. He includes many forays into the land of comparative history, attempting to show that the differences in economic and political development between the two states, especially in the nineteenth century, were more a matter of degree than kind. In the process, he gives a decent account of the domestic events in the United States at relevant points in its relationship with Colombia. This is especially well done for the nineteenth century, during which Washington concluded that it would allow no partners (European or Colombian) to control an isthmian canal. Randall's account of the Panamanian revolution makes clear how the United States (however slowly and inconsistently) used its preponderant power to remove the barrier of Colombian sovereignty from the path to control an interoceanic canal. This work shows how large a gulf was left by this episode in the relations between the two states. Randall also tries to give a Colombian sense of its own views and purposes, reminding the reader that Washington had to deal

not only with another nation-state but another culture as well.

Randall's success in moving beyond traditional diplomatic history is, however, only a partial one. Efforts to explain the sources of United States or Colombian policy almost always come down to some expression of "national interest." Washington wishes to open the door to Colombian trade and to deter rival powers in the Caribbean area. Colombia wishes to protect its domestic producers, expand its foreign trade, and defend its national territory. Randall's efforts to show domestic forces and distinct cultures at work never gets beyond an occasional reference to different perspectives: Washington expected Bogota to welcome closer ties to a nation that could bring it economic and security benefits; Bogota expected Washington to respect its cultural maturity, despite being less rich and stable than the Yankees. This is fine, but the scope of vision is narrow, especially in the coverage of the twentieth century. Very little is said about Washington's desire to "save" Colombia from instability, dictatorship, and European ideas, crusades that, after 1940, led the United States into forceful campaigns to rid Colombia of Nazism and communism—as defined in Washington. Such a role cannot be subsumed under the rubric of national interest.

Little is said as well about the majority of Colombian society. It is the elites (who almost uniformly hold the high posts in government) to which Randall confines his view. Challenges to elite leadership by the rising middle sectors, the organized working class, and later by rural guerrillas are hardly mentioned except as a source of "instability" that hampers Colombian elites from focusing clearly on foreign affairs. Randall never challenges the foreign policy goals of the elites, chiding them only when they lack the realism to accommodate U.S. interests, choosing instead to strike quixotic poses. Nor is Washington's anticommunist perspective challenged. Randall simply treats ideological crusading as an extension of the old goal of stability. Even when he arrives at the cocaine crisis of the 1970s and 1980s, with areas of both countries turned into battlegrounds, Randall maintains his equanimity and his realist perspective, continuing to talk about the benefits of "interdependence" without realizing how much the ground has shifted under his feet.

JULES R. BENJAMIN
Ithaca College

MICHAEL MCCLINTOCK. *Instruments of Statecraft: U.S. Guerrilla Warfare, Counterinsurgency, and Counterterrorism, 1940–1990*. New York: Pantheon. 1992. Pp. xix, 604. \$30.00.

Michael McClintock argues that, in the case of the United States, "terror remains at the heart of the doctrine of unconventional warfare and as such continues to permeate both counterinsurgency and the

ill-defined area of special operations as a whole" (p. 448). His aim is to explicate a policy that has been obfuscated by the use of indirect, unattributable methods shrouded, hitherto, in misleading terminology. He explains that the word "termination," which appears in special forces' manuals, really means "assassination." He assails the apologists' argument that all wars produce terror with the observation that there is no excuse for the use of terror in non-war situations: "a distinction should be drawn between circumstances that beget atrocity . . . and a policy that relies on it" (p. 434).

McClintock maintains that Americans are too often blind to the fact that their own nation is a prime source of terror. They could not see that the U.S. battleship *New Jersey's* "battering of Lebanon's green hills with behemoth shells" in 1983 was the major instance of Middle Eastern terrorism. Yet most U.S. terrorism is perpetrated indirectly through surrogates, by international criminals, and by totalitarian regimes whose police and military have been tutored by American experts in practices such as disinformation, torture, hostage-taking, and assassination. Thus, the acronym "UCLA" took on an alternative meaning during the years when Ronald Reagan was president; "Unilaterally Controlled Latino Assets" were people who "at one remove" could perpetrate crimes for which their American tutors were morally responsible but legally immune (p. 439).

These arrangements stemmed, in McClintock's view, from a misapprehension by U.S. policy makers. Those who shaped national security policy believed that the Soviet Union was expertly deploying the tactics of terror, and that American counter-terror was a necessary reaction. There was a belief that all insurgents whose activities were uncongenial to U.S. policy makers were terrorists, and that terror was their indispensable and most effective tactic. Consequently America became the mirror-image of a phantom, falling in the process into a cesspool of impotent immorality.

McClintock traces the origins of American counterinsurgency to the German experience of antipartisan practices in conquered territory in Russia and France during World War II. He details the ways in which the U.S. Army ingested the lessons, and in some cases the personnel, of Adolf Hitler's attempt to dominate Europe. Yet although he writes with some authority on American anti-Huk stratagems in the Philippines, he believes the "counterinsurgency era" began with the presidency of John F. Kennedy. In terms of tactics and ideology, there would appear to be a stronger case for an earlier starting date, such as that postulated in Douglas S. Blaufarb's book *The Counterinsurgency Era: U.S. Doctrine and Performance, 1950 to the Present* (1977). It is because McClintock thinks in terms of quantifiable numbers of trained men—forty-one units with 7,500 men by Kennedy's death—that he makes his disputable assertion.

Following the Kennedy period of augmentation,

one atrocity followed another. Acting at different times for the army and for the Central Intelligence Agency (CIA), Edward Lansdale engaged in childish, murderous, and counterproductive pranks. One-third of the population of South Vietnam was brutally relocated by 1972. In later years, U.S. officials knowingly, if indirectly, "hired" bombers and also "terminated" Nicaraguan nurses. Yet such was the force of self-deception that the "glamor" of the 1960s Green Berets lived on. So powerful was the "irrational and romantic" conviction that America needed to fight fire with fire that sporadic attempts to mend the totalitarian ways of foreign surrogates met with failure, most noticeably in the case of President Jimmy Carter's human rights campaign. Vicarious terrorism returned with a vengeance with Reagan's presidency. Giving the lie to terrorism's self-proclaimed anticommunist rationale, the end of the Cold War made no difference. By the author's estimate, the "special" forces available to the president increased to 38,400 by 1990.

In places, McClintock is careless. For example, he documents 1990 information with a 1986 source (pp. 349, 555 n. 8). He sheds little new light on the CIA, and he overlooks Cecil B. Currey's biography *Edward Lansdale: The Unquiet American* (1988), which would have been an important source for his study. He makes no convincing attempt to explain why the secret and irregular warriors held unsustainable assumptions about Soviet external terror. But McClintock has uncovered a great deal of new archival material on military covert plans and policy. His critique of postwar American covert doctrine is devastating and is essential reading for any serious student of the history of U.S. national security.

RHODRI JEFFREYS-JONES
University of Edinburgh

ALLON GAL. *David Ben-Gurion and the American Alignment for a Jewish State*. Translated by DAVID S. SEGAL. (The Modern Jewish Experience.) Bloomington: Indiana University Press for Magnes; Hebrew University, Jerusalem 1991. Pp. 280. \$29.95.

The major theme of this study, published in Hebrew in 1985, is the evolution of David Ben-Gurion's policy toward the West during World War II. Portrayed by Allon Gal as the first Zionist leader to comprehend the wartime shift of power from London to Washington, Ben-Gurion became activist and Americentric. After Britain issued the White Paper in May 1939 and the Land Regulations in April 1940, Ben-Gurion implemented "*tziyonut lohemet*"—militant Zionism—in Palestine: illegal immigration, social unrest, and other forms of civil disobedience designed to erode British authority (p. 58). Although careful to avoid aiding the Axis, Ben-Gurion emerged in 1941 as a self-appointed "Zionist preacher" (p. 191), encouraging

the *Yishuv* to resist the worst features of British mandatory policy.

Simultaneously, Ben-Gurion mobilized U.S. Jews to pressure London. He established the American Zionist Bureau in early 1939, enlisted American Jews to lobby journalists and government officials, raised money, recruited soldiers for a Jewish militia, nurtured alliances with fellow activists such as Abba Hillel Silver and important groups such as Hadassah, Poalei Zion, and the Zionist Organization of America, and outwitted others who resisted him. His actions reflected his conviction that Zionists must remain attentive to "the press, the labor movement, churches, and parliamentary and intellectual circles" (p. 187) in Western countries. Ben-Gurion enjoyed dividends in 1941, when the United Palestine Appeal convention endorsed his activist agenda, and in 1942, when the Biltmore Conference echoed his demand for a Jewish commonwealth in Palestine.

Gal's second major theme is the rift that developed between Ben-Gurion and Chaim Weizmann. Compared to Ben-Gurion, Weizmann remained passive and Anglophilic, confident that steadfast loyalty to Britain offered the best means of defeating the Nazi menace and advancing Zionist aims in Palestine. Thus, Weizmann eschewed Ben-Gurion's militant diplomacy for "*shtadlanut*" or "quiet diplomacy" (p. 38) and exhorted fellow Jews with "Chaim's injunction" (p. 51) against anti-British activities. Thus, Weizmann launched political initiatives in Turkey and Saudi Arabia as alternatives to Ben-Gurion's scheming in America, which he feared would disrupt the anti-Nazi struggle. Thus, Weizmann quarreled with Ben-Gurion in late 1940 over the wisdom of challenging London's authority while Britain lay vulnerable to Nazi conquest. "Without England," Weizmann argued, "we cannot make it" (p. 203). Ben-Gurion's triumph at the Biltmore Conference provoked a "stinging exchange" (p. 203) with Weizmann in June 1942 and an enduring split with him thereafter.

Gal impressively bases the development of his themes on research in Israeli, British, and U.S. records. He also discusses cogently, although briefly, the shifting streams of Zionist thinking and politicking in all three countries. Rendered in a sympathetic tone, his judgments about Ben-Gurion's wisdom, ambition, and perceptiveness are reasoned and persuasive. Yet the niche in the literature that this book fills is a narrow one. Gal focuses only on the two themes discussed here, and some of his discussion will seem familiar to readers of Michael Bar-Zohar, Shabtai Tevet, and others.

PETER L. HAHN
Ohio State University,
Columbus

STEVEN Z. FREIBERGER. *Dawn over Suez: The Rise of American Power in the Middle East, 1953–1957*. Chicago: Ivan R. Dee. 1992. Pp. 286. \$26.50.

Had the Suez Crisis never occurred, I am convinced that an enterprising diplomatic historian would have invented it. Using recently opened archival materials at the Public Record Office, the National Archives, and the Eisenhower Library, Steven Z. Freiburger has written yet another provocative account of the affair showing how the United States replaced Great Britain as the senior member of the Anglo-American partnership in the Middle East from 1953 to 1957.

Freiberger argues persuasively that mounting friction between Washington and London over regional defense was more important than Arab-Israeli tensions in triggering the 1956 crisis. Convinced that the Middle East Defense Organization proposed by Britain would provoke anti-Western Arab nationalism and invite Soviet subversion, as early as 1953 Dwight Eisenhower and John Foster Dulles steered toward a Northern Tier defense system stretching from Turkey through Iran to Pakistan. In perhaps the best chapter in the book, Freiburger shows that profound disagreements between Britain and America over the structure and scope of the Baghdad Pact (Should Arab states be pressed to join? Should the Western powers become full-fledged members?) placed Eisenhower and Anthony Eden on a collision course in the Middle East by mid-1955. Eden's insistence that Iraq and Jordan embrace the Baghdad Pact and that Britain become a charter member outraged Gamal Abdel Nasser and drove him into the Kremlin's orbit. It was the Baghdad Pact, Freiburger contends, not Israel's raid on Gaza in February 1955, that led Nasser to accept arms from Moscow seven months later.

Freiberger's account of the period prior to the Suez crisis confirms much of what Keith Kyle, Peter Hahn, and Diane Kunz have already shown. Determined to prevent further Kremlin inroads in the Middle East, America wooed Nasser with the promise of aid for the Aswan Dam and pressed forward with Project Alpha, a top-secret plan for a comprehensive Egyptian-Israeli peace settlement. When Nasser balked at direct meetings with Israel's David Ben-Gurion and the Alpha scheme collapsed, Washington and London adopted Project Omega, a covert plan to isolate Egypt that culminated in the U.S. decision to scuttle the Aswan Dam project in July 1956 with Britain's blessing.

Freiberger offers some provocative but not entirely persuasive insights into the Anglo-American falling out that ensued from Nasser's seizure of the Suez Canal. Personalities loom large here, with Dulles wearing the black hat. According to Freiburger, it was Dulles, not Eisenhower, who torpedoed the Aswan project "because of his personal pique toward Nasser," and who masterminded an American policy of calculated ambiguity by sending "dual bind" messages to the British and then passing misinformation to the White House (pp. 156, 165). Perhaps, but most other accounts of this episode suggest that, for better or worse, Eisenhower was in control. America's pre-

sumed interest in markets in the Middle East and elsewhere in the Third World are emphasized as well. After detailing U.S. efforts at the United Nations to prevent British military intervention at Suez, for example, Freiburger asserts that "it is reasonable to assume that one of Dulles's chief concerns was that should Washington fail to act, Moscow would gain a foothold in the Third World, effectively blocking those markets for American business" (p. 192). Again, perhaps, but assumptions are no substitute for hard evidence, and other scholars have made a more compelling case that Dulles and Eisenhower acted as they did to escape being tarred with the brush of colonialism. Although Freiburger's interpretation is not entirely convincing on these matters, his careful examination of Anglo-American disagreements over the Baghdad Pact and regional strategy makes this study a valuable addition to the rapidly growing library of books on the Eisenhower administration's policies toward the Middle East.

DOUGLAS LITTLE
Clark University

ROBERT P. NEWMAN. *Owen Lattimore and the "Loss" of China*. (Philip E. Lilienthal Book.) Berkeley and Los Angeles: University of California Press. 1992. Pp. xvi, 669. \$30.00.

Several years ago, at the annual meeting of the Society for Historians of American Foreign Relations, John Melby, a former U.S. foreign service officer who got caught in the inquisition of the 1950s, spoke eloquently of the need to keep the memory of those days alive. We must never forget what happened then, he said, so it will never happen again. Robert P. Newman's book should go far in reminding us of the tenuous nature of our freedoms, of how public officials and members of the press can fan the flames of popular passions to a point where the exercise of independent judgment becomes more risky to career and reputation than ignorance, stupidity, incompetence, or venality. Newman has spent the better part of two decades studying and writing about the Asian specialists victimized during the second Red Scare. Here he spends nearly 600 pages of text to tell the story of the man whom Senator Joseph R. McCarthy once labeled "the top espionage agent in the United States."

Owen Lattimore was an extraordinary individual: independent, flamboyant, erudite, courageous, and, yes, a tad arrogant. He never earned a high school or college diploma, yet this did not stop him from becoming the leading Western expert of Central Asia of his generation. He was too bright to be held back by the mere absence of much formal education. The son of an educator who taught for many years in China, Owen at age nineteen hooked up with a British import-export firm in East Asia. His duties took him deep into China's interior, where he soon

developed a fascination and affection for the Mongol peoples who for centuries had struggled against domination by Han Chinese, Russians, and, more recently, the Japanese. Based on his travels, his first book, *Desert Road to Turkestan*, appeared in 1928, when he also returned to the United States. There he was able to secure grants to finance several months of study at Harvard as well as more trips into the depths of Asia. In 1933, with two more books and numerous articles to his credit, he became editor of *Pacific Affairs*, the prestigious journal of the Institute for Pacific Relations. Five years later, he joined the faculty of Johns Hopkins University. A year after that, he became head of the Walter Hines Page School of International Relations there. During World War II, Lattimore spent a good deal of time serving as an adviser to Nationalist Chinese leader Chiang Kai-shek.

Lattimore's myriad travels and activities brought him into contact with individuals from numerous countries and a wide range of political persuasions. Himself a liberal, his writings frequently blended scholarship with current politics at a time when expertise on Asia in the United States was in short supply and ideological battle lines were in their formative stages.

In the aftermath of World War II, Lattimore became increasingly critical of Chiang's rule in China. When Chiang was defeated in the civil war on the mainland, Lattimore argued that the United States should recognize the new Communist regime as a means of encouraging its independence from the Soviet Union. He was far from alone in taking this position, but, joined with controversial stances he had taken in the past, it would soon make Lattimore a leading target of McCarthy and other witch hunters in the U.S. Senate.

Newman takes over 250 pages to tell the story of Lattimore's ordeal from 1950 to 1955. It is a gripping tale, especially from the fall of 1951 onward, when the crusade against Lattimore shifted from McCarthy's shotgun approach to the more systematic efforts of Senator Patrick McCarran and the Justice Department. Newman is by no means a master storyteller. His quotations from Senate hearings, FBI files, and legal briefs are excessive. Yet the drama is so powerful, and the facts unearthed by Newman's often pathbreaking research so eye-catching, that I could scarcely put the book down. On the positive side, the courage and dedication to a cause of Lattimore, his lawyers in the Washington firm of Arnold, Fortas, and Porter, and U.S. District Court Judge Luther W. Youngdahl come forth clearly. On the other side, the nastiness and blindness of Lattimore's persecutors and prosecutors emerge full blown. Newman often wears his outrage on his sleeve, but after reading about the massive effort put forth by public officials to destroy Lattimore it is difficult to quibble with the author's passion.

For a generation whose memory of the second Red Scare must come from sources other than direct

experience, this book should help keep alive John Melby's admonition that the inquisition not be forgotten so it can never happen again.

WILLIAM STUECK
University of Georgia

ROBERT SMITH THOMPSON. *The Missiles of October: The Declassified Story of John F. Kennedy and the Cuban Missile Crisis*. New York: Simon and Schuster. 1992. Pp. 395. \$25.00.

No aspect of John F. Kennedy's presidency has commanded more attention recently than the Cuban missile crisis. Since 1987, scholars and former participants have dissected it at several conferences, the proceedings of which have been published. The government has also opened classified material, including tape transcriptions of Executive Committee (Ex-comm) meetings. All of this has led to several scholarly works on the crisis in the past year.

Robert Smith Thompson has written a critical account of Kennedy's handling of that crisis. Thompson views Kennedy as a cold warrior who reflected both his father's driving ambition and a mindless anticommunist foreign policy that evolved from the Truman era. Following the Bay of Pigs fiasco, the Kennedy administration directed the CIA to eliminate Fidel Castro and to pursue sabotage operations in Cuba. It also considered using U.S. forces there. Fearful of an invasion, Castro invited Nikita Khrushchev to place ground-to-ground missiles in Cuba. The United States deployment of Jupiter missiles in Italy and Turkey had further influenced the Soviet leader. What follows is a detailed account of the thirteen-day crisis. The narrative ends abruptly despite differences that subsequently arose with the Soviets over what other "offensive" weapons should be withdrawn and how removals would be verified.

Thompson questionably asserts that Kennedy had concealed knowledge of the SS-4 missiles in Cuba. Kennedy supposedly wanted the missiles in place in order to sell the American public on the expensive and controversial TFX fighter-plane project. After blaming Kennedy for precipitating the crisis, Thompson then seems to fault him for compromising with Khrushchev by agreeing secretly to removing the Jupiters from Turkey. According to Thompson, "Kennedy's court historians have pretended otherwise, denying that the Jupiters were part of the deal" (p. 355). The fact is that Arthur Schlesinger, Jr., had mentioned the informal arrangement in his biography of Robert Kennedy (*Robert Kennedy and His Times* [1978]), and Theodore Sorensen did so in his editing of Robert Kennedy's posthumous *Thirteen Days: A Memoir of the Cuban Missile Crisis* (1969).

Thompson concludes that the Kennedy administration and the American people have learned the wrong lessons from the crisis. Those lessons include encouraging the country to "hang tough, stay cool,

show flexibility over minor points, but never yielding on major ones; plan to operate with surgical precision, but keep the bludgeon conveniently nearby" (p. 356). The more appropriate lesson, according to Thompson, might be the danger of pride and arrogance, for in the end Khrushchev "beat" Kennedy in Cuba. The continuance of Castro today is proof of that.

The fact is that no one "won" the missile crisis. And perhaps its most pertinent lesson is that both sides could have avoided it. Fortunately, both sides exercised caution in the end. Ironically, even though Thompson uses Kennedy's expression, "The Missiles of October," he fails to explain Kennedy's reference to it. Kennedy employed the term after reading Barbara Tuchman's *The Guns of August* (1962), which revealed how inadvertence had contributed to World War I, a circumstance that Kennedy wished to avert. Today, we know even more clearly how closely the two great powers came to a nuclear confrontation, as Soviet commanders in Cuba had authorization to use tactical nuclear weapons.

This clearly written study is too anti-Kennedy to provide a balanced account. Moreover, it contains innumerable factual errors. For example, Schlesinger, James MacGregor Burns, Allen Nevins, and Sorensen did not write *Profiles in Courage* (p. 67); Kennedy did not "write" *A Nation of Immigrants* in 1960 (p. 90); and John Foster Dulles was not the grandson of Robert Lansing (p. 46).

JAMES N. GIGLIO

Southwest Missouri State University

ADRIAN W. SCHERTZ. *Die Deutschlandpolitik Kennedys und Johnsons: Unterschiedliche Ansätze innerhalb der amerikanischen Regierung*. (Dissertationen zur neueren geschichte, number 23.) Cologne: Böhlau. 1992. Pp. xi, 528.

The literature of American history, especially in its foreign policy aspects and its relations to Germany, has been greatly enriched in recent years by the work of German scholars such as Klaus Schwabe, Detlef Junker, Werner Link, and Reinhard Doerries. Their work, in turn, has stimulated interest in these subjects in Germany. One of the products of that interest is Adrian W. Schertz's study of the policies of the Kennedy and Johnson administrations. Unfortunately, this book does not quite live up to its models.

After the obligatory, but in this case almost useless commentary on methods and sources, and a cursory overview of U.S. policy toward Germany up to 1960 (which actually devotes a page or so to the period before World War I), Schertz launches into an extensive but indiscriminate discussion of U.S. policy toward Germany during the years Dean Rusk served as secretary of state. His organizing principle, which provides the subtitle for the volume, is the alleged conflict in the formulation of that policy between the

Europe and Germany experts of the State Department and the "globalists," drawn largely from the East Coast academic elite, whom Kennedy brought with him to Washington. Rusk, who, according to Schertz, "saw himself primarily as loyal supporter of his president but, at the same time was close to his Europe experts, veered back and forth between these groups and, in the end, drew criticism from all sides" (p. 462).

It is indicative of the analytic confusion this produces that two of the most prominent examples of Schertz's "globalists" are a Nebraska lawyer (Theodore Sorensen, whose principal influence on Kennedy in any event came from his speech writing) and a California-born Midwest industrialist (Robert McNamara, far more the technocrat than the policy formulator), and that among the "experts" are listed both Walt Rostow, as much of an eastern academic and early Kennedy supporter as the "globalist" Arthur Schlesinger, and Henry Kissinger, another Eastern academic, whose mainly unsolicited memoranda during these years played no role worth mentioning in the formulation of American policy.

It would have been far more useful to ignore the "different approaches" and to concentrate instead on the actual policies pursued, in the formulation and execution of which the "globalists" Kennedy and Johnson frequently relied on old Germany experts such as Dean Acheson and John J. McCloy. Schertz should have explored more fully the essence of the U.S. foreign policy of which its policy toward Germany was only a part, albeit an important one, and the changing circumstances that dictated alterations in U.S. policy regardless of generational factors or of whose advice was listened to. It might also have helped had Schertz paid more attention to the German part of the equation and resisted the temptation to look on every difference of opinion as a major crisis.

As it stands, the volume is useful as a checklist of issues and actions that comprised the U.S.-German relationship, but it does not significantly clarify any of them.

MANFRED JONAS
Union College

THOMAS W. ZEILER. *American Trade and Power in the 1960s*. New York: Columbia University Press. 1992. Pp. xiv, 371. \$45.00.

"The Cold War is over, and Japan has won." This was the observation of Democratic Party presidential hopeful Paul Tsongas in early 1992. Denouncing Tsongas as a Japan-basher, Governor Bill Clinton noted that his primary challenger's observations were too final. "We have lost battles in the trade war," Clinton insisted in the 1992 New Hampshire primary, but "the war" continues. Although Tsongas won the New Hampshire primary, Clinton won the presidency. Both men placed dramatic focus on a "war"

that received scant attention from the American political community and its electorate during the post-World War II ideological confrontation with communism. What is the nature of this "war," and how did it begin? Did America ever really hope to "win" it, and, if so, why? Some of these questions lay at the foundation of Thomas W. Zeiler's competent study of U.S. trade policy in the "can do" days of the New Frontier and the Great Society.

As the Tsongas-Clinton debates suggest, Zeiler's work was published at a most relevant moment. Yet the topic of U.S. trade policy, of course, is more complex than those debates imply. Zeiler admits that trade negotiations are the most involved within the preserve of diplomacy. His subchapter headings help the reader sort through the often tedious procedure, but this approach has the countereffect of limiting analysis and creating a certain trade policy textbook that was not the author's intention. For instance, one of the architects of modern Japanese trade policy, Hayato Ikeda, receives brief mention in only two areas of the book, including a two-and-one-half page subchapter, "Japan and United States Trade" (pp. 82-84). The effort of nations, such as Japan, to take advantage of America's preoccupation with the Cold War in favor of national economic self-interest would have been a tempting and valuable discussion here, but the book quickly moves on to new subchapters, for example, "Protectionism and the LTA" (pp. 84-86).

Without question, a larger work can be accomplished in the effort to explain America's reluctance to take world economic diplomacy seriously. In any event, this larger mission was not Zeiler's mission. Instead, he has given us a fine account of a confused economic giant at the height of its political and military power. Zeiler should be thanked for presenting this detailed and masterfully researched book that fills a wide gap in the study of modern economic diplomacy, and for taking on an intricate topic that has been avoided for too long by American diplomatic and economic historians.

TIMOTHY P. MAGA
Bentley College

CANADA

BARRY M. GOUGH. *The Northwest Coast: British Navigation, Trade, and Discoveries to 1812*. (Pacific Maritime Studies Series, number 9.) Vancouver: University of British Columbia Press. 1992. Pp. xiv, 265. \$39.95.

On July 12, 1776, as the first British empire collapsed, Captain James Cook set off on his third Pacific voyage, a key event in the articulation of Britain's second empire. Cook's instructions required that he put in at Francis Drake's New Albion landfall just above San Francisco Bay and then proceed northward, charting the coast along the way, in search of the ever-illusive Northwest Passage. Despite his scientific aims, Cook's most significant discovery was com-

mercial and largely accidental. In March 1778, he sailed into Nootka Sound off the west coast of Vancouver Island and was met by native peoples eager to trade sea otter pelts for European manufactures, especially metal goods. Although Cook died on the Pacific crossing in Hawaii, his men soon learned that the pelts brought enormous prices in China. The scramble for control of the Northwest Coast was on.

Barry M. Gough's valuable study details that scramble in a survey of Britain's efforts to explore the coast and chart it precisely, to assert control over it, and to exploit its commercial potential by developing the Pacific fur trade. The effort demanded extraordinary exertion and skill on the part of crews and ships working far from home ports in a challenging environment for years at a time, and it often produced intense conflict with Spanish, American, and Russian merchant-explorers with similar aspirations. Despite occasional setbacks, the British were largely successful. By the second decade of the nineteenth century the Northwest Coast had been integrated into the world economy, and Britain's claim, while not yet fully secure, was well established.

Gough's approach is in the grand tradition of the history of discovery and is resolutely European in focus. This is not to say that he ignores the aboriginal presence. Gough reports the opinions of various Britons on native culture and deplors the impact of the European invasion on Indian peoples. Indians are secondary to his story, however, for their assigned role is passive, and there is little effort to explore the region's gradual integration into a European orbit from their perspective. This is unfortunate. As Edmund Burke pointed out in 1790, when a dispute between England and Spain over this "distant dominion" almost led to war, the region belonged to neither European claimant but to its aboriginal inhabitants (p. 141). This is not a matter of abstract justice, but of power. The Northwest Coast remained "Indian country" well into the nineteenth century, a place where native peoples controlled the rules of exchange and where their alliances and aspirations shaped the outcome of contests between the several European powers. One cannot fully understand the European conquest of the region without attending to native behavior in the face of the invasion. Still, this is a valuable book, engagingly written and thoroughly researched, a compelling description of the Northwest Coast from the British viewpoint.

RUSSELL R. MENARD
University of Minnesota,
Twin Cities

LARRY A. GLASSFORD. *Reaction and Reform: The Politics of the Conservative Party under R. B. Bennett, 1927-1938*. Buffalo, N.Y.: University of Toronto Press. 1992. Pp. xiv, 308. Cloth \$45.00, paper \$19.95.

Richard Bedford Bennett was prime minister of Canada for one term (1930–35) and left the Conservative Party weaker when he stepped down in 1938 than it was when he took on the leadership in 1927. Because of this apparent failure, the Bennett government has been negatively perceived and largely ignored by scholars. Larry A. Glassford's book is not the long-awaited biography of Bennett, nor is it a close analysis of government policy, and it is certainly not a postmodern evaluation of the contested terrain in which Bennett functioned. Instead, it is a highly readable narrative study of the struggles within the national Conservative Party under Bennett's leadership "to adapt its program, organization and image to meet the needs of a modernizing Canada" (p. ix), and as such it fills a large gap in Canadian political history.

According to Glassford, the seeds of Bennett's failure were planted during the leadership convention of 1927. The Conservative platform was developed with an eye to harmony rather than consistency, with the result that divisive issues relating to the tariff, English-French dualism, and the distribution of wealth in an industrial economy were never fully resolved. Although Bennett quickly developed as an effective leader in the House of Commons and used his personal fortune to finance a modern party organization, he was unable to create an effective cabinet "team." The laissez-faire faction in the party clashed with the Bennett paternalists, and both sides were too concerned with defending the social order and maintaining balanced budgets for the populists. Faced with mounting hostility from the public at large and a populist revolt within his party, Bennett launched a "new deal" campaign in the winter of 1935, promising such reforms as unemployment insurance and minimum wage legislation. Confused voters abandoned the party in the 1935 election.

Glassford maintains that Bennett's new deal was the logical conclusion of the Conservative legislative program rather than a departure from it, but it is difficult to accept such a position. Certainly the so-called laissez-faire members of the Conservative Party did not believe it as they moved to curb their leader's rhetoric during the election campaign. In his desperate last-minute attempt to play midwife to the welfare state and economic management, Bennett caused anxious moments for those who financed the party, not because they opposed state intervention but because they opposed this brand of it. Moreover, it is difficult to accept Bennett as the victim of contending forces within the party as he is portrayed by Glassford. Even when he had Franklin D. Roosevelt, another wealthy paternalist, as a model, Bennett could not be a convincing reformer.

Although this study does little to alter the larger assessment of Bennett's government, it provides useful insights on Conservative Party organization in this period. It also offers a long-range perspective on tensions that continue to plague the party, which since 1942 has trumpeted its dual nature by adopting

the apparent oxymoron "Progressive Conservative" as its official name.

MARGARET CONRAD
Acadia University

PAUL LITT. *The Muses, the Masses, and the Massey Commission*. Buffalo, N.Y.: University of Toronto Press. 1992. Pp. viii, 331. Cloth \$50.00, paper \$19.95.

The Massey Commission on National Development in the Arts, Letters, and Sciences (1949–51) enjoys mythical stature in modern Canadian history. Mandated to recommend federal government policy in the field of Canadian culture, it is credited with rescuing Canadian universities from financial penury and saving scholars, artists, writers, and poets from cultural oblivion. Indeed, some admirers see the report as Canada's most successful royal commission study ever. Paul Litt's masterful analysis of the commission's activities will force some reconsideration of its historical significance. He does not set out to slay sacred cows, but he tells the commission's story so meticulously that both its contributions and limitations come sharply into focus.

Litt ably documents the political agendas, hidden and otherwise, that lay behind the commission's work. Yet its members—diplomat and philanthropist Vincent Massey, academics Hilda Neatby, Georges-Henri Lévesque, and Norman Mackenzie, and the "outsider," businessman Arthur Surveyer—all took their jobs seriously and by no means behaved, at least not consciously, as partisan pawns. Their mission was to deliver "high" culture to the Canadian population in a palatable form, no mean feat in an environment in which a Vancouver newspaper columnist could write: "We in B.C. are proud of our modern artists. True, we cannot understand them. But this is not necessarily a disadvantage. In fact if we *could* understand our artists, we might not be so proud of them" (p. 59).

Such attitudes provide a clue to the commission's strategy. It sought to seduce its skeptics and "confound its enemies" (p. 40) by linking the support of culture to patriotism. Opponents of high-quality, state-supported radio and television programming were painted as apologists for an American-dominated media and entertainment industry that threatened to undermine liberal humanism and impede the development of indigenous cultural expression in Canada.

That the commission's report was favorably received by a public whose indiscriminate tastes and values had been the object of the commissioners' disdain was testimony both to a postwar mood of nationalism and to the careful crafting and marketing of the Massey Report itself. Litt's sensitive sketch of the context in which the report was conceived and issued reveals much about the buoyant expectations and enduring insecurities that pervaded Canada in the years following World War II.

In terms of cultural policy, the report was, at best, a qualified success. Its proposal that the federal government fund universities was adopted by Ottawa well before the report was published, and the creation of the arts-funding agency, the Canada Council, more than five years after the report was completed, owes more to serendipity than to the direct influence of the Massey Commission. Of such coincidences are myths fashioned.

Readers may have difficulty with Litt's loose use of the term "cultural elite," a rubric that appears to include every Canadian in the first half of the twentieth century who believed in and lobbied for state support for the arts. From this perspective, small-town library promoters who procured tiny government grants would be part of the elite, but wealthy capitalists who patronized the arts while eschewing direct involvement with government would not. Litt appears uninterested in the work of other scholars who study elites, class relations, or policy development, although some of this literature might have usefully informed the conceptual framework of this study.

Despite these lapses, the book's even-handed and literate treatment of the royal commission's labors renders it a major contribution to the study of Canadian intellectual and cultural history in the postwar period. Notwithstanding the author's refusal to perpetuate the Massey myth, the commissioners themselves, given their mission, would likely have agreed with this positive assessment of the book.

PAUL AXELROD
York University

LATIN AMERICA

PHILIP P. BOUCHER. *Cannibal Encounters: European and Island Caribs, 1492–1763*. (Johns Hopkins Studies in Atlantic History and Culture.) Baltimore: Johns Hopkins University Press. 1992. Pp. xii, 217. \$32.95.

Increasing interest in the pre-European culture of Amerindian peoples, and their response to the colonizers, makes Philip P. Boucher's book a welcome addition to a growing field of literature. Unfortunately, little is known about the origins and culture of the Island Caribs, but Boucher presents a range of interesting hypotheses, especially questioning the widespread European belief in extensive Carib cannibalism. While Boucher is frustrated about the lack of information about indigenous Carib culture, he does devote most of the book to showing us that Caribs were not passive elements on whom Europeans acted but, rather, followed a recognizably rational policy in their efforts to obtain desired European technology, to play off the European occupiers against one another in an effort to preserve their own independence, and to adopt European military practices in order to achieve their ends. At the same time, he documents the generally negative view of Europeans toward the Caribs, among the adventurers who

actually encountered them and even more among the educated elite. He also offers an explanation for the generally more positive French relations with the Caribs, in contrast to the prevailing hostility between the Caribs and the English.

From the first Columbian encounters, Caribs were seen as both hostile and brutal, and popular European travel literature reinforced this perception. At the same time, beginning in the 1620s, northern Europeans began to reside in the Lesser Antilles, and Boucher details their growing presence and the growth of the French and English empires there. The French policy of *douceur* toward the native Caribs, directed from the metropole, gained them some friendship among the Caribs, who tended to see the English as the greater enemy. At the same time, and central to Boucher's thesis, the Caribs, as best they could, tried to limit European encroachments, and did so through a version of what Boucher calls "Realpolitik Caribbean Style," supporting whatever policy might best help them in maintaining their autonomy and their land. In the end, the generally pro-French tendencies of the Caribs did not result in much Carib support for the French against the British, nor did the French policy of *douceur* bring greater benefits for the Caribs than the harsher British policy. By the late eighteenth century, the Caribs were a vanishing people.

Boucher concludes with a discussion of the Caribs in European literature through the Enlightenment. He argues that the French view was more positive than was the English, although many French authors continued, despite contrary evidence, to describe the Caribs as brutish cannibals. It is surprising that Boucher fails to mention the treatment of the Caribs in Aphra Behn's *Oroonoko* (1688), a sympathetic depiction by an English author, very different from negative stereotypes in Daniel Defoe's *Robinson Crusoe* (1719), which he discusses at some length. Despite this quibble, however, the book is a strong contribution to our understanding of the interplay not only between France and Britain in the struggle for the Antilles but also between the colonizers and the indigenous people fighting to maintain their independence from both European powers.

AMY G. GORDON
Denison University

K. LYNN STONER. *From the House to the Streets: The Cuban Woman's Movement for Legal Reform, 1898–1940*. Durham, N.C.: Duke University Press. 1991. Pp. xi, 242. Cloth \$42.50, paper \$16.95.

K. Lynn Stoner's book is a welcome addition to the literature on Latin American women. It joins the books by Anna Macías, June Hahner, Francesca Miller, and Marifran Carlson that document the small but active feminist movements that existed throughout Latin America in the first half of the twentieth

century. This growing body of works makes the early movements to improve women's education and legal rights the most thoroughly researched subject in Latin American women's history. Taken together they demonstrate that, although ignored by later historians, these movements are essential to understanding the intellectual ferment and social movements of the period.

Stoner provides an excellent reconstruction of the Cuban women's movement from 1898 to 1940. She focuses on the four main women's organizations, their leaders, journals, strategies, and causes. She also analyzes congressional debates on the reform of divorce, adultery, legitimacy, and suffrage laws. Her research is impressive. She used Cuban archives and libraries as well as the more easily accessible materials in the United States. She collected a wide array of written sources, including personal correspondence, memoirs, and the minutes of the first three Cuban women's congresses. She also interviewed the surviving leaders of the feminist movement.

The story she pieces together shares many similarities with those of other Latin American countries. Most feminist leaders were well-educated, white women from privileged social backgrounds. Their ideas were closer to European rather than U.S. feminism, what Karen Offen has termed relational rather than individualistic feminism. They emphasized motherhood, protection of women workers, and community needs more than gender equality. Still, the Cuban case is distinctive in some ways. For one, the early feminist movement was tamer in Cuba than in Argentina (a difference that begs explanation). For another, the Cuban movement—largely coinciding with the revolution of 1933—benefited from the large-scale mobilization of the 1920s and 1930s that gave women's groups some leverage with contending politicians.

Stoner's book is notable for going beyond the "heroic women" genre in several ways. Instead of painting a unified picture of women, she emphasizes the ideological divisions and conflicts among the different feminist organizations, which were conservative as well as liberal and radical. She recognizes the antifeminism of some politically active women. She notes the distance between most feminists and lower-class women. Moreover, she attempts to expand the limited focus of studies of Latin American feminists by analyzing some aspects of the private lives of sixteen Cuban leaders. Although she does not provide full biographies, her chapter on "The Prosopography of the Feminist Leadership" raises intriguing questions about the links between their political activism and their university education, high rate of spinsterhood, and lack of "respectable" job opportunities.

The main weakness in Stoner's study comes in assessing the impact of the Cuban feminist movement. Although she considers it highly influential, her occasionally imprecise presentation makes it hard

for the reader to judge. For example, although we know that a handful of leaders were active over many years, we have no sense of the size of the membership or the degree to which these organizations existed apart from their leaders. Since the leaders she studies lived in Havana, we are left wondering whether a feminist movement existed in the provinces. Thus, when she states that the elite Cuban feminists were not representative of the majority of Cuban women, we are not told whether they even represented a majority of elite women. (If not, could this explain the ease with which later generations forgot their achievements?) And, when Stoner acknowledges that the women's organizations could not take all the credit for major legal reforms and progressive labor legislation, the reader must question if she has not overstated their impact in general. Nonetheless, specialists in Cuban and women's history will find this an interesting and valuable study.

SILVIA MARINA ARROM
Brandeis University

TERENCE N. D'ALTROY. *Provincial Power in the Inka Empire*. Washington, D.C.: Smithsonian Institution. 1992. Pp. xii, 272. \$42.50.

One of the salient features of the successful invasion of the Andes by only a few hundred Spaniards was the apparent lack of resistance by the Inca who ruled a territory thousands of miles long and controlled a population of millions. Among the explanations for such success was the early collaboration of the Wanka, a polity in the central Andes. The Wanka provided the newcomers with food, troops, bearers, and insights into Inca rule. The Europeans felt so secure in their dealings with the Wanka that they set up their first capital in their territory, at Jauja. Where the Pizarro brothers encountered prolonged resistance, as at Huánuco, it was Wanka allied troops who were brought in to break up the Inca holdout.

For the first thirty years of European rule the lords of the Wanka felt themselves apart from other traditional Andean rulers: they not only converted very early to Christianity but their sons also learned Spanish and traveled to Spain, where they were received at court and granted coats of arms. Later, when European authorities attempted to treat them as "Indians," they sued in colonial courts, backing their claims with data from traditional *quipu* knot records. It was only thirty years after the invasion that a Wanka lord joined other "Indians" to demand that European immigration be stopped and traditional ethnic rule be restored, if under overall Hispanic sovereignty.

Terence N. D'Altroy summarizes here the prehistory of the Wanka as revealed by archaeological surveys and excavations over a decade or more. He claims not to be impressed by what is revealed about Wanka and Andean history from Spanish written sources. Instead, he sees archaeology as a better door

to comparability among archaic empires. "Archaeology and documentary sources are to be treated as independent lines of evidence; we cannot assign meaning to archaeological data from historical sources and then treat the archaeology as verification of ethno-history . . . This does not deny importance to ideology in Inca rule but suggests that we will gain insight into the overall nature of empire best if we look first to the energetic and material and second to socially shared concepts" (p. 14).

Actually, in practice, extensive use is made by D'Altroy and his team of archival materials on such fundamental themes as the absence in the Andes of trade, the reality of a decimal administrative vocabulary, and the possible simultaneous use by a given ethnic group of geographically distinct territories. But because comparability on a world-wide scale is the stated aim, unique Andean features are subordinated. Thus, the report by one of the European administrators most familiar with and curious about Andean institutions, lawyer Juan Polo Ondegardo, that he had for many weeks fed close to two thousand European troops with what was still stored in the Jauja warehouses sixteen years after the invasion, is referred to only in passing. Yet it raises questions that seem to be at the heart of issues such as "tribute" or "markets" in Inca economics or statecraft.

The most convincing section of the book is the detailed archaeological verification of the change in Wanka settlements from mountaintops in immediately pre-Inca times of endemic warfare to valley occupations under Cuzco rule. This brought with it not only *pax incaica* but also resettlement in zones of increased productivity, at lower altitudes. A new role for the production and storage of prized maize is documented archaeologically.

The Inca are defined as a people with a non-market, centrally planned, and dendritic system, with strong vertical ties. D'Altroy's intent is to move away from explanations of the Inca empire in its own terms (pp. 215, 219). The effort now is to look for common ground with the economies of other early empires that had no markets.

JOHN V. MURRA
Institute of Andean Research
New York

ALIDA C. METCALF. *Family and Frontier in Colonial Brazil: Santana de Parnaíba, 1580–1822*. Berkeley and Los Angeles: University of California Press. 1992. Pp. xvi, 280. \$40.00.

Alida C. Metcalf is to be congratulated on her well-crafted analysis of Brazilian family life on the São Paulo frontier. This community study ranges from the sixteenth to the early nineteenth centuries and successfully contextualizes the analysis within the larger frameworks of Brazilian and world history. Metcalf is both interdisciplinary and comparative in

her methodology, and she demonstrates a strong command of the literature in family history and anthropology. Families are divided by socioeconomic group: planter families (who owned slaves); peasant families (who had no slaves); and slave families. This organization is quite effective since the personal and economic issues for each of these groups are different, and there is a specific thematic literature in each area. Metcalf also sees geography as an important dimension of her study: scholars in the area of "comparative frontiers" will be interested in Metcalf's analysis of the ways in which the frontier both created and maintained social inequality, even while migration functioned as both a personal and a family strategy for status maintenance.

Much of the originality and liveliness of the book is due to Metcalf's use of record linkage of wills, property inventories, censuses, and some parish records to follow individuals and families over time. Individual actions are interpreted in the light of family relationships and resources. For the planter families the focus is mainly on the way in which property is passed between generations and divided among siblings. Changes over time in household and family composition and even in occupations is most notable for peasant families. In the case of slave families Metcalf shows how individual families are affected by death and marriages within the planter family that owns them.

Controversy will arise in response to Metcalf's view of elite inheritance practices. Brazilian inheritance law mandated equal partition of property among heirs (except for the uncommitted third). Most Brazilianists believe this law was scrupulously followed by eighteenth and nineteenth-century judges. Metcalf claims that families "almost always" favored daughters and that parents chose one heir who was eventually to inherit their position.

Some aspects of this argument are valid. The not uncommon use of the dowry enhanced by the third in the seventeenth century has already been noted, but as Muriel Nazzari has argued, the favoritism toward daughters was balanced by the marriages of sons to equally dowered daughters of other families (*Disappearance of the Dowry* [1991]). Migration as a family strategy to conserve resources has also been shown in other contexts. Migration, however, did not affect the legal position of an heir. Maybe the heir who migrated would have been willing to sell his or her share of particular family properties to other heirs for a money equivalent, but inherit that person would.

Most of the data on elite inheritance are presented in anecdotal form. Even though Metcalf collected 241 property inventories, the appendix explains that only nine property inventories from the general collection and eleven more that reached the appeals court were useful for analysis of planter inheritance patterns. One begins to wonder how representative this sample could have been. At least Metcalf should have told us how many of the twenty-one wills favored a single

heir and if there were a difference between the nine from the general collection and the eleven from the appeals court. Most important is the fact that this evidence is limited to families who made wills. In fact, many families—the majority, including elite families—did not make wills and all of their property was divided equally between heirs. It only made sense for someone to make a will if they wished to deviate from the norm and divide property nonequally.

My greatest reservations concern Metcalf's conclusions that census descriptions of heirs' households twenty or thirty years after the division of property is proof of parental favoritism. Metcalf argues that selling shares between family members "allowed one heir to inherit the position of the parents" (p. 111). But what evidence is there that this was an intentional occurrence? It may have simply been that one heir was more industrious, more successful, or made a better marriage and therefore was able to buy out the shares of siblings. The personal industry of the heir's household is entirely discounted in such an analysis as is the mortality or sale of slaves that may have been inherited.

Nevertheless, this book has great appeal. It is vividly written and conveys a strong sense of frontier life in colonial Brazil. The chapter on the slave family is particularly insightful. Students will especially like and profit from it. If scholars fail to agree with all of Metcalf's conclusions, they will nevertheless be fascinated by her dynamic use of the sources and particularistic approach to human agency in the form of family strategy.

ELIZABETH KUZNESOF
University of Kansas

TIMOTHY R. SCULLY. *Rethinking the Center: Party Politics in Nineteenth- and Twentieth-Century Chile*. Stanford: Stanford University Press. 1992. Pp. xiv, 287. \$42.50.

Timothy R. Scully's book is a thoughtful and probing study of center parties in Chilean politics. It balances respect for the Chilean case with concern for the broader themes and claims of comparative analysis. It challenges comparativists and Chileanists alike on many points.

Scully argues that the roles of center parties depend on their specific identities and programs and on their relations with other parties and therefore they must be determined empirically. He looks at center parties in three "critical" junctures in Chilean history. In the first of these, in the mid-nineteenth century, Liberals stepped between warring clerical and anti-clerical groups, helping to shape the country's first party system and deflecting political energies in more constructive directions. In the 1920s and early 1930s, with the clerical issue receding and industrial workers pitted against their employers and the bourgeois

state, the opportunistic Radicals became the linchpin of a new party system, allying with one or the other side as opportunities arose.

Both parties, Scully argues, accommodated divergent constituencies by addressing the vestiges of earlier cleavages while mediating the polarizations developing around new ones. So, too, he thinks, did the Christian Democrats (PDC) in the 1950s and early 1960s, when they rose to prominence with the support of mobilized peasants, agricultural workers, and urban slum dwellers recently arrived from the countryside.

Scully is at his best in analyzing the political dynamics of particular periods. His treatment of the second half of the nineteenth century makes wonderful sense of divisions and antagonisms that in most accounts remain capricious and inscrutable. His coverage of the 1930s and 1940s, while based on secondary works, bursts with the flavor and authority normally confined to observers or participants.

He is less persuasive in dealing with the concept of the political center and with the PDC's rise to power. Regarding the political center, he appears to believe that parties fill certain objective "spaces," that is, center, center-left, and center-right (which, alas, he does not define), and yet at other times he implies that such space is actually a function of the support levels for left-wing and right-wing alternatives.

In attributing the PDC's rapid expansion to the agrarian question, however, he cites its organizing efforts among agricultural workers in rural areas in the 1950s and offers electoral data from the 1950s and 1960s. In doing so, he underplays the extraordinary personal appeal of its leader, Eduardo Frei, whose presidential campaign in 1958 did not include agrarian reform, but whose efforts to develop a national and popular movement extended well beyond the party's ranks. In addition, the electoral data (pp. 128–29) do not prove the salience of agrarian concerns in the Christian Democratic surge. In fact, they show the Right retaining most of its strength between 1953 and 1957, the Left outpolling the PDC between 1957 and 1961 in both rural and urban electoral districts, and the PDC catching fire (equally in rural and urban districts) between 1961 and 1965, when other issues might have been equally or more important.

If Scully wanted to attribute specific motives to the people supporting the Christian Democrats, he should have looked at available survey data (Eduardo Hamuy's for 1958 and 1964) for Santiago, where many of the new voters and displaced rural workers could be found. But even so, he has written a valuable, informative, and artfully crafted book that comparativists and Chileanists will enjoy reading and rereading for years to come.

MICHAEL FLEET
Marquette University

Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed.

GENERAL

JUAN R. I. COLE, editor. *Comparing Muslim Societies: Knowledge and the State in a World Civilization*. (The Comparative Studies in Society and History Book Series.) Ann Arbor: University of Michigan Press. 1992. Pp. xiv, 328.

JUAN R. I. COLE, Introduction. NIKKI R. KEDDIE, Material Culture, Technology, and Geography: Toward a Holistic Comparative Study of the Middle East. CHARLES LINDHOLM, Quandaries of Command in Egalitarian Societies: Examples from Swat and Morocco. DALE F. EICKELMAN, The Art of Memory: Islamic Education and Its Social Reproduction. ALLAN CHRISTELOW, The Muslim Judge and Municipal Politics in Colonial Algeria and Senegal. CHARLES D. SMITH, The Intellectual, Islam, and Modernization: Haykal and Shari'ati. ELLIS GOLDBERG, Smashing Idols and the State: The Protestant Ethic and Egyptian Sunni Radicalism. DENIZ KANDIYOTI, Women, Islam, and the State: A Comparative Approach. SHAHROUGH AKHAVI, Shi'ism, Corporatism, and Rentierism in the Iranian Revolution.

MICK GIDLEY, editor. *Representing Others: White Views of Indigenous Peoples*. (Exeter Studies in American and Commonwealth Arts, number 4.) Exeter: University of Exeter Press, for AmCAS. 1992. Pp. xi, 152. £12.95.

MICK GIDLEY, *Representing Others: An Introduction*. STEPHANIE SMILES, *A Native American in Stone: The Simcoe Memorial in Exeter Cathedral*. TIM YOUNGS, *The Medical Officer's Diary: Travel and Travail with the Self in Africa*. ANTHONY FOTHERGILL, *Of Conrad, Cannibals, and Kin*. RON TAMPLIN, *Noble Men and Noble Savages*. PETER QUARTERMAINE, *Johannes Lindt: Photographer of Australia and New Guinea*. MICK GIDLEY, *Edward S. Curtis' Indian Photographs: A National Enterprise*. RICHARD MALTBY, *John Ford and the Indians; or, Tom Doniphon's History Lesson*.

EMILIAN POPESCU *et al.*, editors. *Études byzantines et post-byzantines*. Volume 2. (Académie Roumaine.) Bucharest: Editura Academiei Române. 1991. Pp. 228. 500 L.

EMILIAN POPESCU, *Basilique et synagogue dans le Sud-Est de l'Europe à l'époque protobyzantine (IV^e-VI^e siècles)*.

ADRIAN RĂDULESCU, *Recherches archéologiques récentes dans le périmètre de la cité de Tomis*. IOAN BARNEA, *Sur les rapports avec Byzance du territoire situé au Nord du Bas-Danube durant la période Anastase I^{er}-Justinien I^{er} (491-565)*. DAN GH. TEODOR, *Éléments et influences byzantines dans la civilisation des VI^e-VII^e siècles après J. Chr. au Nord du Bas-Danube*. PETRE DIACONU, *Sur l'organisation ecclésiastique dans la région du Bas-Danube (dernier tiers du X^e siècle-XII^e siècle)*. TUDOR TEOTEI, *L'opposition entre les notions de "monarchie" et "polyarchie" à Byzance (IX^e-XIII^e siècle)*. STELIAN BREZEANU, *"Mésiens" chez Nicéas Choniata: Terminologie archaisante et réalité ethnique médiévale*. VICTOR SPINEI, *La signification des ethnonymes des Daces et des Gètes dans les sources byzantines des X^e-XV^e siècles*. ȘERBAN PAPACOSTEA, *Byzance et la création de la "Métropole de Moldavie"*. OCTAVIAN ILIESCU, *Les armoiries de la ville d'Asprokastro et leur origine byzantine*. ERNEST OBERLÄNDER-TÄRNOVEANU, *Moldavian Merchants and Commerce in Constantinople in the 15th Century in the "Book of Accounts" of Giacomo Badoer*. EMANUELA POPESCU-MIHUȚ, *Remarques sur la place des textes de droit criminel byzantin dans la pratique judiciaire roumaine du XVIII^e siècle*. VALENTIN AL. GEORGESCU, *L'impact de la "Loi agraire" byzantine sur les projets de Code Rural révolutionnaire (1791) et napoléonien (1801-1814), disparu du schéma de la codification modernisatrice du droit roumain au XIX^e siècle*. MARIN COJOC *et al.*, *Études et recherches de byzantinologie des six dernières années*.

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GIORGIO CHITTOLINI and DIETMAR WILLOWEIT, editors. *Statuti città territori in Italia e Germania tra Medioevo ed Età moderna*. (Annali dell'Istituto storico italo-germanico: Quaderno, number 30.) Bologna: Mulino. 1992. Pp. 502. L. 50,000.

GIORGIO CHITTOLINI, Statuti e autonomie urbane: Introduzione. DIETMAR WILLOWEIT, Città e territorio nel Sacro Romano Impero: Una introduzione. GERHARD DILCHER, Diritto territoriale, diritto cittadino e diritto dello Stato principesco. ELENA FASANO GUARINI, Gli statuti delle città soggette a Firenze tra '400 e '500: Riforme locali e interventi centrali.

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The following books were recently received in the *AHR* office. Books listed here do not include works scheduled for review.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editor's discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

ARTICLES

TO THE EDITOR:

Now that the *AHR* has devoted 44 pages [98 (April 1993): 338–81] to disquisitions about the pertinence of Michel Foucault's writings for modern history, may we expect allocation of similar space in a future issue to deliberations about the relevance for some historical subject of the writings of Nostradamus?

HENRY A. TURNER, JR.
Yale University

TO THE EDITOR:

I read with great pleasure Carole Shammas's excellent article, "A New Look at Long-Term Wealth Inequality" (*AHR*, 98 [April 1993]: 412–31). It posed intellectual challenges, foremost among them the relationship between income and wealth inequality. This is a topic that needs far more attention: which of the two measures is valuable for which purpose and why?

Shammas has chosen to deal only with wealth inequality and considers how wealth and wealth inequality have been measured. My comments are on one aspect of the measurement of wealth: how in the evaluation of domestic wealth *should* the domestic wealth of nonresident foreign investors be handled? This matter is important for several reasons: if the main purpose of measuring wealth inequality is "to measure economic power," as Shammas argues (p. 421), then clearly such foreigners' wealth must be included; second, to understand how a nation's wealth is distributed, it should also be included; third, to develop national tax policies, it should be included; but, fourth, if the main purpose is to consider how

residents, as distinct from outsiders, control national wealth, then it ought to be excluded.

However, no matter what the purpose, the wealth of nonresident foreign investors will not show up in domestically probated wills. (It does appear, however, in the probated wills of the nonresidents, for example, in British probated wills.) I had the same difficulties with Alice Hanson Jones's data that depended on American wills as did Shammas (see n. 38, p. 631, and nn. 105–06, p. 638, of my *History of Foreign Investment in the United States to 1914* [1989]).

When I was seeking (for my study of the history of foreign investment in the United States) to determine the level of nonresident British investment in this country on the eve of the revolution, which I believe would be the equivalent to those investors' American "wealth," I used the same claims data that Shammas did. There are, it should be noted, two sets of British claims data: one relating to commercial debt and the second to real and personal property. She used only the latter; some of the commercial debts represented long-term obligations and thus "wealth."

Yet I believe it is not the claims that are the appropriate yardstick but rather the compensated claims. Because claimants often exaggerate losses, unlike Shammas (see p. 429 of her article), I chose to use compensated claims data. The "allowed" claims were far lower than the actual ones. In my *History of Foreign Investment in the United States to 1914* (pp. 26–27), I tried to evaluate nonresident foreign investment as a percentage of "colonial domestic wealth." My denominator—following Jones—excluded foreign-owned "domestic wealth."

Determinations of wealth (domestic or foreign-owned) are very tricky, as Shammas knows. She accepts the estimate (with no footnote) that the holdings of the Penns were £10,000,000 (see p. 418). However, information in the *Loyalist Transcripts* (in the New York Public Library manuscript room) indicates that the Penn family claim came to £944,817. The Penns were eventually awarded £500,000 (see my book on this). Shammas's own data on pp. 429–30 certainly suggest that the £10,000,000 figure she gives is out of line. How should one determine the value of the Penn family's American holdings, their American wealth?

I want to congratulate Shammass on her exciting article. Clearly, she has opened the way for a collection of doctoral dissertations—and ultimately an important book.

MIRA WILKINS
Florida International University

CAROLE SHAMMAS REPLIES:

Mira Wilkins, who has written extensively on foreign or nonresident investment in the United States, asks why I used the allowed claims in my article rather than the actual compensation amounts in estimating the wealth of nonresident loyalists, types who are unlikely to appear in the probate records used by Alice Hanson Jones in her estimations of colonial wealth on the eve of the revolution. I used the allowed claims because Parliament sharply discounted its compensation for the wealthy loyalists, the ones of interest for my calculations, and it is apparent from the account of the commissioners (John Eardley-Wilmot, *Historical View of the Commission for Enquiring into the Losses, Services, and Claims of the American Loyalists*, 1815, reprinted 1972, pp. 69–78) that this discounting was done according to a formula based on the service given to the crown, the economic condition of the applicant (the richer you were, the smaller the percentage of the entire claim compensated), and the financial condition of the nation, not on the validity of the claims themselves, which the commissioners, before sending on to Parliament, pared down considerably according to set criteria. Consequently, the claims allowed by the commissioners seemed a better estimate of the wealth of loyalists in the top 1 percent than did the actual compensation.

While I feel fairly confident that the allowed claims rather than the compensated amounts are the correct ones to use, I by no means consider my 1774 figures to be definitive. A much more elaborate analysis, using the Audit Office Papers in the Public Record Office and transcripts of those papers in the New York Public Library, as well as materials relating to the commercial debts claimed by British merchants and other property compensated for by others or not confiscated, would produce a more accurate figure. It would also, I have little doubt, increase the proportion owned by the top 1 percent. That is why, elsewhere in the article, I suggest the 1774 wealth inequality figures are still an underestimate.

Finally, the source for the 1760s estimate of the Penn fortune is Lorenzo Sabine, *Loyalists of the American Revolution*, and it is footnoted in the article at the end of the paragraph where the reference to the Penns appears.

CAROLE SHAMMAS
University of California,
Riverside

TO THE EDITOR:

In her article on long-term trends in wealth inequality (*AHR*, 98 [April 1993]: 412–31), Carole Shammass argues that measurements of household wealth should exclude the present value of future Social Security benefits. Her reasons for excluding such benefits are, first, that historically they did not primarily replace savings, but rather they replaced “living with relatives, working longer, and collecting military pensions,” and, second, that “[t]hey cannot be marketed or traded or used to exercise economic clout” (p. 421).

As to the first point, if an elderly person is able to stop working or to continue living independently because she can rely on receiving Social Security benefits, then clearly she is wealthier than she would have been without the benefits, and no measurement of her wealth that excluded this improvement would be accurate. True, if the Social Security benefits merely replaced military pensions, there would be no improvement. But the aggregate value of military pensions in 1860, one of the chief points of comparison with data from recent years, was probably close to zero.

As to the second point, about marketability, Shammass and I could easily write a contract by which I would sell her today the right to receive from me, at the appropriate time, a stream of payments equivalent to my Social Security benefits. Even if the courts refused to enforce such contracts, the result would not be a complete cancellation of the wealth value of Social Security benefits but merely a marginal reduction in their value.

The increased “economic clout” of the elderly in the post–World War II era can hardly be doubted. It is evidenced in part by the increased *political* clout of the same age group. (A chance to retire also means a chance to be active politically.) Clearly, one reason for this change is the availability of Social Security.

RICHARD JOFFE
New York City

CAROLE SHAMMAS REPLIES:

No, no. I will be happy to sell Richard Joffe the rights to my Social Security benefits. I also have a bridge . . .

CAROLE SHAMMAS
University of California,
Riverside

REVIEWS OF BOOKS

TO THE EDITOR:

In her review of my *Lion in the Lobby: Clarence Mitchell, Jr.'s Struggle for the Passage of Civil Rights Laws* (*AHR*, 97 [December 1991]: 1636–37), Paula F. Pfeffer re-

veals the types of revisionist errors that commonly characterize civil rights histories. A primary reason for the confusion is the lack of understanding of the National Association for the Advancement of Colored People's role in leading the movement.

It was therefore not surprising that even the most friendly reviews of the book failed to recognize the full significance of the NAACP's signal accomplishments in persuading both the executive and legislative branches of government to join the Judicial Branch in upholding the Constitution for the protection of the rights of African Americans, and thus other minorities, women, native Americans, and the handicapped. In order to appreciate the importance of that accomplishment, it is necessary to understand, first, that Clarence Mitchell, Jr., was to the legislative struggle what Thurgood Marshall was to the judicial struggle: the guiding force and spirit. Marshall's vision enabled the NAACP to achieve its historic victory in getting the Supreme Court to overturn the "separate but equal" doctrine in its landmark decision in 1954 in *Brown vs. Board of Education*. The decision reasserted the original meaning of the Fourteenth Amendment, which accorded full citizenship to African Americans.

Under *Brown*, Mitchell noted, the nation at long last recognized that discrimination and segregation were one, a position to which the South was violently opposed because the implications were obvious. The nation could no longer tolerate state-imposed segregation without destroying the Constitution or undermining its role as leader of the free world. Mitchell heralded *Brown* and used it to undergird his struggle in Congress for passage of laws to enforce the decision. Three years later, he got Congress to pass the first civil rights law since Reconstruction. The 1957 Civil Rights Act was primarily a weak voting rights law. Even so, it broke the psychological barrier against such measures and enabled Mitchell subsequently to win passage of the 1960 Civil Rights Act, the 1964 Civil Rights Act, the 1965 Voting Rights Act, and the 1968 Fair Housing Act, and all the related strengthening provisions up until his retirement from the NAACP in 1978. *Lion in the Lobby* shows how he won those victories.

Pfeffer fails to appreciate not only Mitchell's mastery of the legislative process but also his unique ability to appeal to human decency even among opponents. His messianic zeal and unsurpassed skills enabled him to marshal bipartisan support to defeat formidable southern opposition. He won support from conservative congressmen such as William Knowland and Thomas Kuchel, both of California, who were House minority leaders of the bills at respective periods. Support from other conservative Republicans—Representative Clarence Brown of Ohio and Senator Everett Dirksen of Illinois—was equally crucial.

Pfeffer's ignorance of the legislative aspects of the civil rights struggle is especially obvious from her complaint that "in one place" in the book, the efforts

of presidents Dwight Eisenhower, John Kennedy, and Lyndon Johnson are praised, "while in another they are condemned for insufficient concern for civil rights." At the same time, she declares of the author, "his admiration for President Lyndon Johnson is such that he thinks it necessary to cover up the role played by the NAACP in Johnson's election in 1964" (p. 1637).

Anyone with a fair knowledge of civil rights history knows that, even though President Roosevelt issued Executive Order 8802 in 1941, which barred discrimination in the defense industry and created the Fair Employment Practices Committee, he was not a friend of civil rights. Subsequently, pressured by the NAACP, presidents Truman, Eisenhower, and Kennedy supported aspects of the struggle. As Senate majority leader, Lyndon Johnson was a southern moderate on civil rights; in the White House, he provided the type of presidential leadership that Mitchell sought. Support from these presidents was progressive, recognizing the social and international pressures of the periods in which each served.

Pfeffer is confused about the purpose of *Lion in the Lobby*, which is a biography that seeks to present the most salient aspects of Mitchell's life in understandable terms for the reader, rather than the NAACP's role as an institution. For example, Mitchell was central to any decision within the NAACP on how to help Johnson beat Arizona Senator Barry Goldwater, the Republican standard-bearer, in the 1964 election; nevertheless, it was the entire national NAACP structure, meaning its national board of directors and Wilkins, that was responsible for the decision, as the page one story in *The New York Times* showed.

Mitchell's legislative priorities were determined by the civil rights complaints he received primarily through NAACP branches. He sought no new rights for African Americans, as some historians have claimed, only that they should be extended the same rights accorded to others under the Constitution. He justified his demands for the safeguards he sought from Congress and the Executive Branch on constitutional and moral grounds, rather than on social and political ideology or philosophical concepts.

Whether Republicans or Democrats were in office, Mitchell found that progress was made, as he said, "because of the political power of the people." Those who "sought to turn back the clock" had been "busy under all administrations," he said. They "failed to place the stamp of final approval on segregation because the NAACP" had "met them at every crossroad and alley-way." Mitchell, of course, was one of the leaders who met them there. As a tribute to his vision and genius, he was popularly known in Washington as the "101st senator."

DENTON L. WATSON
Freeport, New York

Paula Pfeffer was offered an opportunity to respond. She replied that she stood by her review as written.

THE EDITOR

TO THE EDITOR:

An author, I believe, is usually best advised not to respond to a review of her or his book, however perverse the criticism may sometimes appear. Such things must be accepted in good spirit as part of the hazards of the profession. But Berenice A. Carroll, in her review (*AHR*, 98 [February 1993]: 140–42) of my books *Freedom from Violence: Sectarian Nonresistance from the Middle Ages to the Great War* and *Freedom from War: Nonsectarian Pacifism, 1814–1914* goes beyond the usual scholarly critique to question both my legal conduct and my professional ethics. I therefore feel these two issues require at least brief comment.

With respect to Carroll's insinuation that I have infringed the copyright held by Princeton University Press on my earlier volumes, *Pacifism in the United States: From the Colonial Era to the First World War* and *Pacifism in Europe to 1914*, I would like to state that Princeton University Press was shown virtually the whole manuscript of my present work and that it was at the suggestion of its then Executive Editor for Humanities (in a letter dated January 30, 1989) that I soon afterward approached another university press in the matter of publication.

Stating she is "puzzled at the absence of any mention of the . . . extensive overlap" with my volumes published by Princeton, Carroll goes on, "Whatever one may think of this in terms of scholarly ethics," etc. etc. (p. 140). She fails, however, to mention (is this perhaps a "deliberate omission"?) that in the bibliographies of each of the books she is reviewing, my Princeton volumes are both cited in full. The titles involved indicate clearly that considerable overlapping is inevitable, for the overall subject dealt with and the time span are basically the same. Surely, too, it is unreasonable in these circumstances to demand (as Carroll does) that I should add to my already extensive endnotes a long series of references to myself?

PETER BROCK
University of Toronto

BERENICE CARROLL REPLIES:

Peter Brock's letter has only underlined the problematic issues raised by his unacknowledged reuse of extensive portions of his previous works in his two recent volumes. It is unfortunate that Brock has not offered in his response any clarification of his own intentions in publishing these two new volumes drawing so extensively on his earlier works.

I made no "insinuations" but stated explicitly that I find it perplexing and disappointing "that Brock fails to offer any explanation of the relationship between these two volumes and the earlier works." I am happy to note that Brock confirms my stated assumption that he had the permission of Princeton University Press for publication by another press. Had this been acknowledged in the new volumes, it would not have called for any comment.

I regret that Brock feels my criticism of his work was "perverse," but I continue to find it questionable that the extensive use of this earlier material is nowhere acknowledged in these two volumes. The fact that Brock's earlier works are listed in the bibliographies of the new books does not constitute such an acknowledgement. The titles certainly do not make obvious the extent of the word-for-word overlap. On the contrary, the titles suggest significant differences in both framing and content, which do exist. It took many hours of my own time and the time of my assistant, Nada Elia, to determine what those differences were.

As Brock notes, I used the phrase "whatever one may think of this in terms of scholarly ethics"—meaning that scholars may *disagree* about the ethics of it. But Brock's "etc. etc." sidesteps the consequences I delineated: "the practical problem is that Brock leaves entirely to the reader the tedious work of page-by-page and note-by-note comparisons, whether for research or teaching purposes."

This remark was not a "demand" that Brock provide "a long series of references" to himself. But it is not at all "unreasonable" to expect an author to direct readers to his or her own earlier and fuller treatment of a subject, either through a general statement in a preface or introduction, or through endnotes, or both. Brock has done this in the case of chapters based on journal articles (an example is noted in my review). If Brock did not wish to offer "a long series of references" of this kind to his earlier books, a single summary note for each chapter where the overlap was extensive would have sufficed.

It is regrettable that Brock's silence on this matter has directed attention away from the new chapters and other valuable additions that do appear in his new works. The bulk of my review took pains to indicate to readers what the major changes were, as well as what I view as substantive limitations of these volumes. I would hope that, having aired my criticism and his defense on the irritating issue of "overlap," we and others can turn the dialogue to those substantive issues concerning the varieties and character, past and future, of pacifism and nonviolence.

BERENICE A. CARROLL
Purdue University

TO THE EDITOR:

Allow me to comment on your decision to use Ronnie Dugger as a reviewer for Robert Dallek's recent biography of Lyndon B. Johnson, *Lone Star Rising* (*AHR*, 98 [April 1993]: 460–63).

First, full disclosure: Dallek and I are good friends, and we have collaborated in several professional ventures. I have read and admired his LBJ book, as well as most of his other work. Certainly, Dallek does not need me to defend either the quality or integrity of his biography, both of which Dugger has assaulted. Interested scholars can easily learn about such mat-

ters by reading Dallek's book or by making their own inquiries.

Let me be perfectly clear: I have no quarrel with Dugger; it is, rather, with you. Your decision to use Dugger is simply wrong. To his credit, Dugger candidly acknowledged that "the editors of the *AHR* dismissed my written expostulations that I might well be deemed to have a conflict of interest" (p. 460). Bravo for Dugger. He is an LBJ biographer whose view and research strategies are decidedly different from Dallek's; clearly, Dugger is a most interested party.

Dugger had it right at the outset, and that should have been the end of the matter. But no, craving blood, needing your prurient interest satiated, looking for some games, or whatever, you "dismissed" his injunction and convinced him to write the review—and satisfied all your longings and lusts.

Enough. Well, almost enough. I appreciate the difficulty of selecting proper reviewers. Some of my best friends are book review editors. On occasion, I am sure, they innocently select a reviewer itching to cream an author or a disciple/friend/acolyte of that author. Mistakes happen. But this decision was made consciously—perhaps maliciously. At best, it was dumb. Shame.

STANLEY I. KUTLER
University of Wisconsin,
Madison

RONNIE DUGGER REPLIES:

Anent Stanley Kutler's letter attacking the editor of the *AHR* for selecting me to review Robert Dallek's book on Lyndon Johnson:

In discourse on serious matters one knows something about, one is not prohibited from commenting because one has a personal interest in the subject, as long as that interest is divulged. This well-known rule was properly illustrated by this journal's publication of my review of Dallek's book. Were it not for this same well-known rule, Kutler could not properly have written his letter of attack nor could the *AHR* properly have published it. What then is Kutler's point?

Surely it cannot seriously be that the editor, "craving blood, needing [his] prurient interests satiated, looking for some games, or whatever, . . . satisfied all [his] longings and lusts." What a marvel it would be, if a scholar in Madison, Wisconsin, could so uncannily divine the sanguinary and passionate longings and lust of an editor in Bloomington, Indiana. I have always held to their credit the fact that universities are randy places—but psychic?

Perhaps Kutler, wishing to manifest his virtuous loyalty to his friend, but realizing that while this would be exemplary, by itself it would not be persuasive, has simply spun it up into a cotton-candy tirade. Far better, in defense of his friend, had Kutler not inflated my criticisms of Dallek's book into assaults on

its quality and integrity, which they were not, and had written instead a short series of expository sentences describing where and in what ways he believes I was unfair to his friend's work.

RONNIE DUGGER
Wellfleet, Massachusetts

TO THE EDITOR:

I would like to respond to J. Lionel Gossman's curious review of my book, *In the Public Eye: A History of Reading in Modern France, 1800–1940* (*AHR*, 98 [April 1993]: 509–10), on two related points, one apparently minor, the other more obviously substantive.

The first matter concerns Gossman's translation of Jean-Paul Sartre's "Est-il seulement possible de penser à quelqu'un au passé?" (*La nausée* [Paris, 1938], 94). I am afraid that Gossman has taken as much liberty with Sartre's text as I have. In Lloyd Alexander's rendering (*Nausea* [New York, 1964], 63), the question reads, "Is it possible even to think of someone in the past?" Gossman's choice of "past tense" for "passé" is, of course, appropriate to the final paragraph of his review, but I still prefer my less literal translation. My version emphasizes the existential issue of Roquentin's relationship with Anny in and through the text of her letter, the point that I was making on pages 196–97 in my book. Perhaps Gossman will permit me (and Alexander) another interpretative slant.

The second matter also concerns the last paragraph of Gossman's review. There he states, "In the end, however, it is reading that moves the literary scholar, as opposed perhaps to the historian, not the history of reading." That assessment is all too true, since literary specialists, theorists especially, prefer ahistorical approaches to texts and their interpretation, one reason why Paul Ricoeur and Hans-Georg Gadamer have been so critical of Jacques Derrida. My book was intended, in part at least, to address the problem of ahistoricity among our colleagues in literature and sometimes even intellectual history. On this and another issue that Gossman raises concerning my book's focus, let me quote a key paragraph from the conclusion: "An historical understanding of reading . . . offers significant insight into the dialogic constraints on interpretive subjectivity, especially in the negotiation actually undertaken by readers in the past with literature and the world around it. The specific conditions affecting this relationship over time resulted in interpretive practices defined by contemporary communities of readers, by the development of literate culture, and by the evolution of literature itself. These factors cannot be studied in isolation from one another. The simple tracing of taste, as long practiced by literary historians, has never entirely succeeded in explaining interpretation. Nor, for that matter, has an exclusive focus on the text, intertextuality, or discourse, as emphasized by other literary specialists. The most important work on literary and

theoretical audiences, especially those defined within the text, would benefit greatly by a more empirical, more broadly historical perspective. There remains as much to know about actual readers and their perceptions, past and present, as there is to discover about apparent readers and their roles in literature. Until now, few scholars have bothered to pose even the most obvious questions about reading: who read what, where, when, how, and why? The present history proposes some tentative answers for modern France. Its approach suggests what this new knowledge of documented audiences and their reading has to offer the disciplines of literature and history alike" (p. 313).

The differences between Gossman and myself over translating Sartre's question may seem trivial, but they actually reveal, I fear, fundamental differences between us over the challenge to historical thought in contemporary scholarship on literature. Given the disturbing tendency of some postmodernists (and their proponents in intellectual history) to read away the past, I really wish that Gossman had made this issue clearer in his review. It is, after all, a major implication of my book.

JAMES SMITH ALLEN
Southern Illinois University,
Carbondale

J. Lionel Gossman does not wish to reply.

THE EDITOR

TO THE EDITOR:

I suppose I should not be surprised that Walter D. Kamphoefner concludes his review of *Popular Narratives and Ethnic Identity* (AHR, 98 [April 1993]: 585–86) by claiming that "the best clues to ethnic identity remain what immigrants did and what they wrote home rather than what they read"; immigrant letters are, after all, the object of Kamphoefner's research, although not of mine. Still, his logic escapes me. Even if reading didn't leave the kind of archival traces that Kamphoefner is able to understand, wasn't it something that immigrants "did"?

I certainly plead guilty to not having written the kind of straight-up, empirical history that he seems to regard as the only legitimate scholarly undertaking. But why it was necessary to belittle the project with arguments that are often no more substantive than a disparaging remark about nineteenth-century typography ("valiantly slogging through *Fraktur* type") is unclear, unless someone once criticized Kamphoefner for wasting his career studying poor penmanship. There is, however, more at stake here than details, and the issue should not be joined with cheap shots and innuendo. By presuming to judge how much "tolerance of literary jargon" historians might need to read the book—actually, not much—Kamphoefner shows that he is either unwilling or unable to participate in debates over ethnicity and its produc-

tion or in the discussions of subjectivity, identity, difference, and marginality that have been so productive in literary and cultural studies. Admittedly, this is not the kind of language one would encounter while transcribing and annotating letters, but it should not be beyond Kamphoefner's vocabulary. If he really believes this is simply jargon, it is no wonder that German-American studies is widely regarded as one of the last bastions of narrow-minded provincialism.

BRENT O. PETERSON
Duquesne University

WALTER KAMPHOEFNER REPLIES:

One would hope that Brent Peterson is more accurate in quoting the *Abendschule* than in quoting my review. What I wrote was: "the best clues to ethnic identity remain what immigrants did and what they wrote home rather than what they *may or may not have read*." Popular (or in this case, unpopular) narratives, like immigrant letters, can indeed offer valuable clues to ethnic identity. But the better one reconstructs the context in which they were written, the greater their historical value. Reading does leave archival traces if one bothers to look in the right places. This could have led to an interesting exploration of the reasons why the *Abendschule* initially evoked comparatively little resonance with its intended audience and reached its peak of subscriptions at a time when, according to Peterson, its rationale for existence had practically disappeared. But of course that would be a rather underwhelming result (after diligently slogging through all that *Fraktur*), compared to discovering the key to ethnic identity that has somehow escaped historians all these years.

WALTER D. KAMPHOEFNER
Texas A & M University

TO THE EDITOR:

In wartime, historians have been known to join or even lead the cheering for Our Side. Later, though, in tranquility, they reexamine their commitment—or so one hopes. The historian who remains, half a century later, mired in the shallow, mendacious, penny-dreadful literature of wartime does no credit to the profession and risks making a serious fool of himself. Please allow me to comment on Warren Schiff's review of my book *The "Nazi Menace" in Argentina, 1931–1947* (AHR, 98 [April 1993]: 618).

Schiff is not a Latin Americanist; his review reveals his unfamiliarity with the last twenty to thirty years of historiography of great-power relations in the Western Hemisphere. However, he has dispensed with the reviewer's customary protocol—description, historiography, sources, evaluation of craftsmanship, and such fripperies—to express his indignation with me for proposing that, in the light of documentation now available, the received journalistic-cum-State Depart-

ment wisdom concerning Argentina in the 1930s and World War II might just possibly require reconsideration.

He begins by asserting: "[Newton] establishes the barely tenable thesis that the Nazi threat in Argentina was dealt with too heavy-handedly. But he pays scant attention to more thoroughgoing considerations." Please note the passive voice and unstated assumptions: *who* "dealt" with Argentina? *Whose* "thoroughgoing considerations"? Official Washington, obviously (for British diplomats thought American "considerations" regarding Argentina ridiculous). I reply, first, that the United States dealt not only "harshly" but also arrogantly, ignorantly, and counterproductively with Argentina—hardly novel propositions, among scholars at least. Secondly, the book is not written from a U.S. perspective at all. There were *four* principal actors: the governments of Argentina, Great Britain, Germany, and the United States; I have attempted to write from a hypothetical point *equidistant* in space from Buenos Aires, London, Berlin, and Washington. The British were by far the shrewdest; the Argentines the trickiest and most resourceful; the Germans and Americans the clumsiest and most prone to self-delusion. A *fifth* point of view is also represented, that of refugees and exiles on La Plata. To me, it is the most sympathetic.

Finally, Schiff's "thoroughgoing considerations" take for granted the supreme right of the United States to make moral judgments, on whatever grounds or none at all, concerning any people or regime in the Western Hemisphere or the world that displeases or defies it, and to inflict whatever punishment it chooses (or can get away with) on transgres-

sors. In view of the history of U.S. interventions in the hemisphere, Southeast Asia, the Middle East, and elsewhere, do we need to argue *that* proposition again?

If the reviewer disagrees with a "thesis," one expects him or her to attempt a point-by-point refutation. No such thing: Schiff's denunciations are blanket denunciations. His only challenge to a specific point is his rebuttal of a throwaway opinion in the "Introduction," to a line that refers to the 1840s and is already qualified with a "probably." This is irrelevant to a book about the 1930s and 1940s. The remainder of the review is centered on the "Introduction." The 380-odd pages of text and 80-odd pages of notes that follow seem to have escaped his notice.

Warren Schiff's review will not shake the conviction of educated Latin Americans that they are better informed about the United States than their counterparts in the United States are about them—that, indeed, the commonest gringo stance toward them is one of ignorance compounded by ethnocentric and often utterly balmy moralism.

RONALD C. NEWTON
Simon Fraser University

WARREN SCHIFF REPLIES:

Ronald Newton's comments lack focus, coherent pertinence, and momentous new evidence concerning my critique. Therefore, his statements merit no further discussion.

WARREN SCHIFF
College of the Holy Cross

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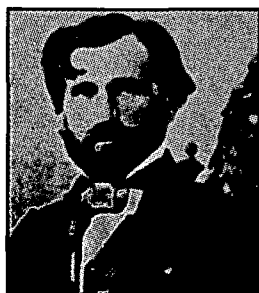
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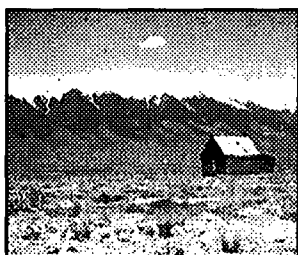


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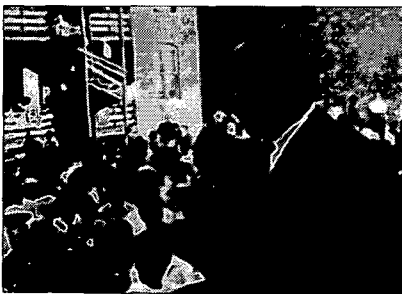
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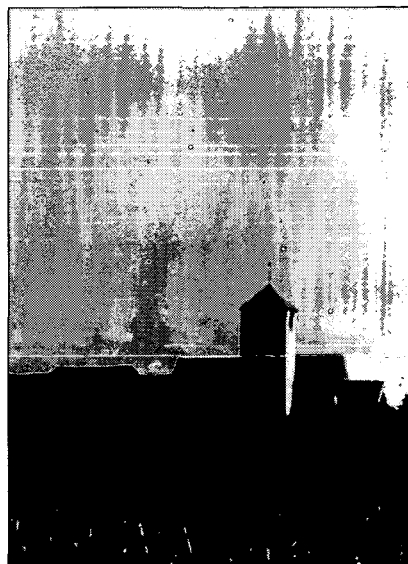
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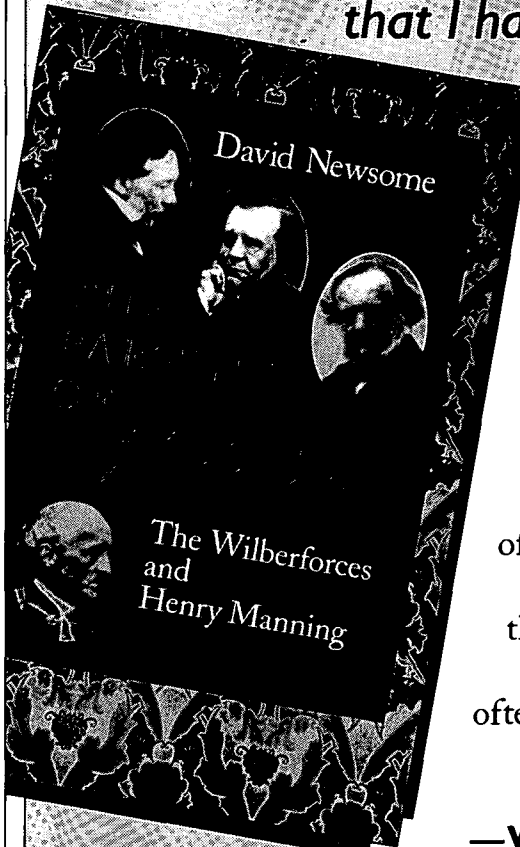
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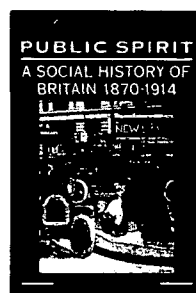
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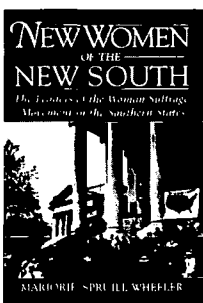
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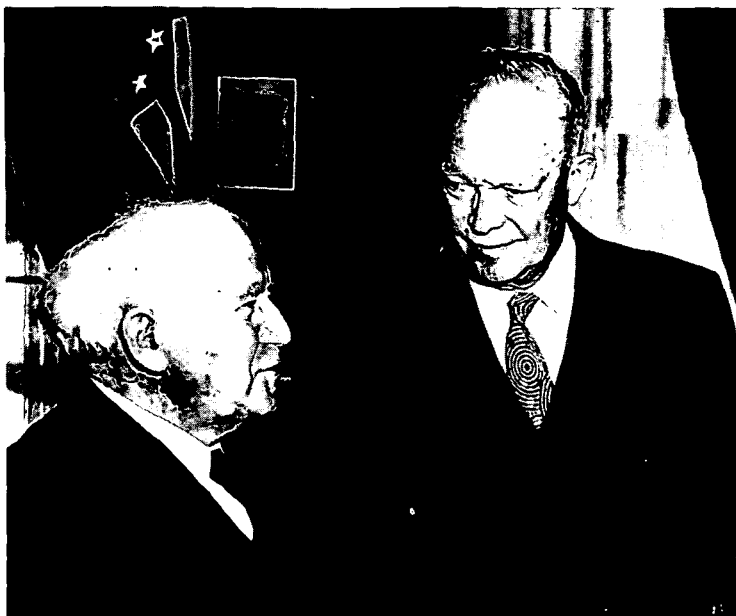


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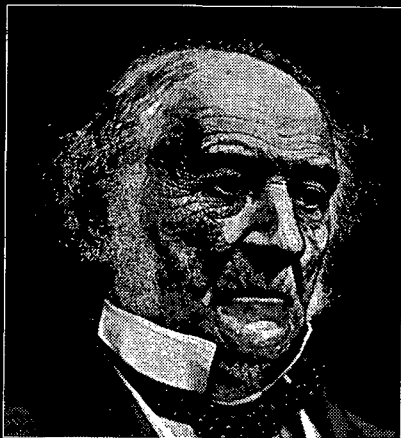
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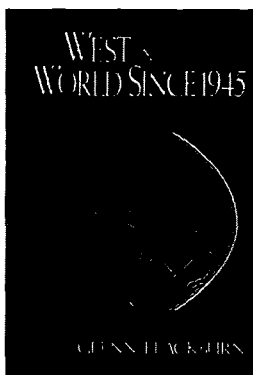
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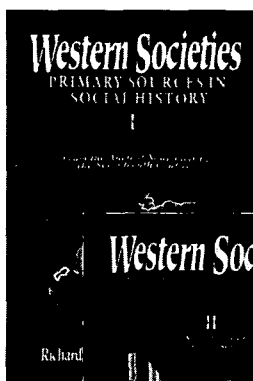
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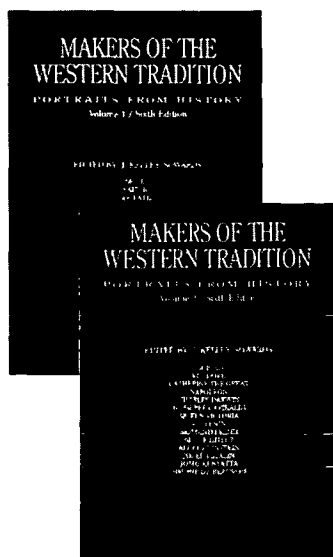
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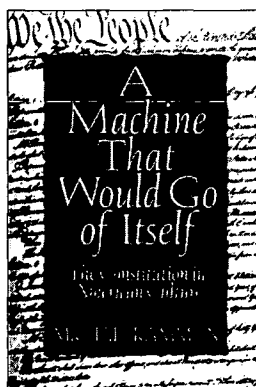
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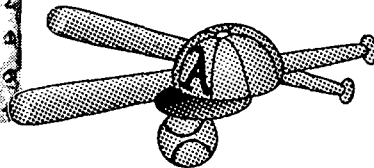


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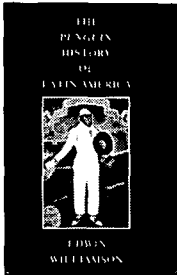
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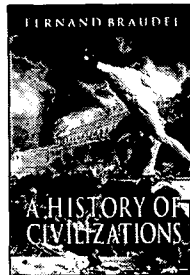
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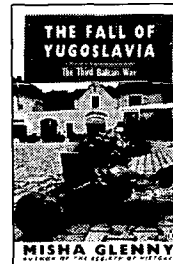
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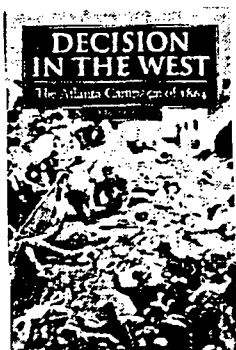
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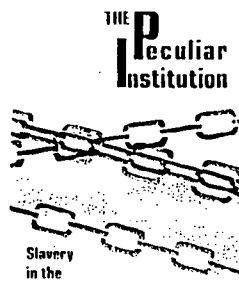
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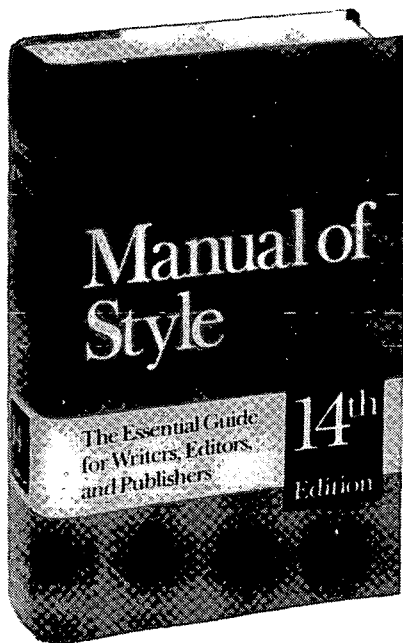
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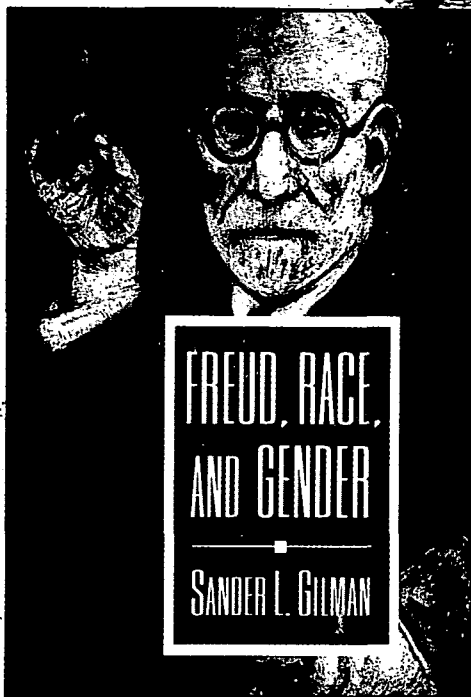
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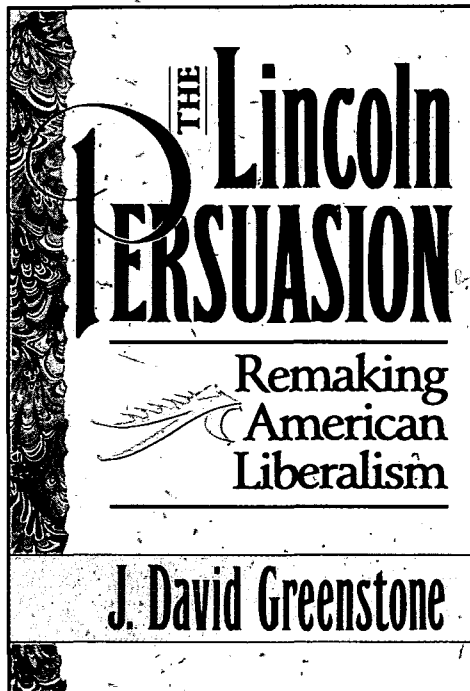
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